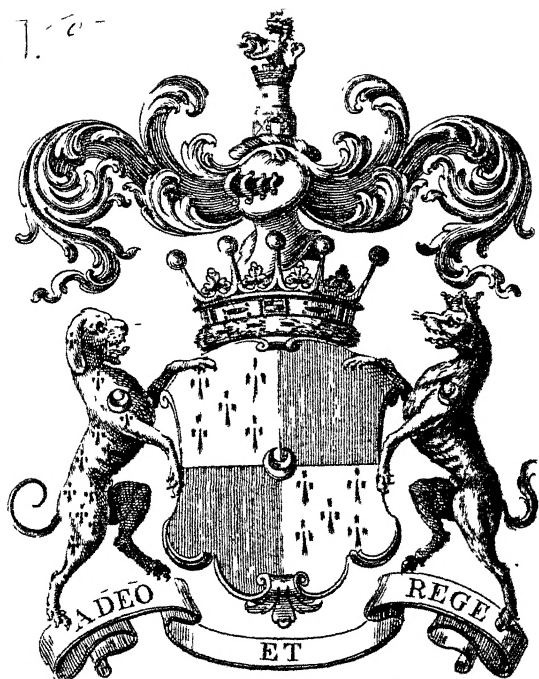ADEO ET REGE

Philip Earl Stanhope.

AGRICULTURAL RESEARCH INSTITUTE

PUSA

PHILOSOPHICAL

TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Prefent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

VOL. LI. PART I. For the Year 1759.

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TO

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E R R A T A.

P. 96. *in the Title to* Art. XVII. *for* 1757, *read* 1758.
P. 285. *l.* 4 *from the bottom, after* damaged, *add,* not.
P. 394. *l.* 8. *before* late, *insert* the; *after* of, *dele* the.

PHILOSOPHICAL
TRANSACTIONS.

I. *The greateſt Effect of Engines with uni-*
formly accelerated Motions conſidered. By
Francis Blake, *Eſq*; *F. R. S.* *

Read June 24,
1756.

THE writers, I have met with,
upon the *maximum* of Engines,
or the greateſt effect poſſible in
any given time, have ſuppoſed the working parts of
the machine to retain their direction, and be uni-
formly moved by the force of a current. They
have therefore conſidered only the caſe of an uniform
Rotation, as in the action of grinding; where the
impediments and impulſes being brought to a ba-
lance, the impulſes are but ſufficient to prevent a
decay in the generated motion. And, upon that
view of the Problem, the load of an Engine, when

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* The diſtance of time betwixt the reading of this paper, and
its now appearing in the *Tranſactions*, was owing to the abſence
of the author, and his deſire to reconſider it before it went to
the preſs.

B

the effect is a maximum, and the force a current, is determined by computation to be *four ninths* of the weight which would caufe the Engine to reft. This, then, being fuited only to an uniform velocity both in the lever and obftacle, I would confider the cafe of an uniformly accelerated one in repeated vibrations. The maximum which correfponds to it is adapted to the fteam-engine, and of no lefs importance to be determined than the other. And, no doubt, thefe fpeculative inquiries into mechanical fubjects, were they even barely confidered as our guides in experiment, may appear to deferve the attention of the Society. But indeed, and I ingenuoufly confefs it, an opportunity to make fome remarks upon what I formerly laid before you, concerning the proportion of the cylinders, was a further inducement to the prefent refearch.

A general expreffion for the time of a ftroke in fuch vibratory Engines, will lead us without trouble to a computation of their effects.

$$\underset{m}{\text{A}} \qquad a \qquad \underset{\wedge}{\text{C}} \qquad b \qquad \underset{n}{\text{D}}$$
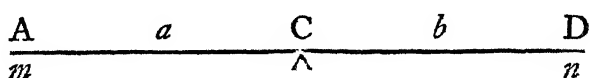
Let A D be a lever, whofe Brachia are a and b, and fuppofed without weight. Let m be a Power, and n a weight. Then $a : b :: n : \frac{bn}{a}$, the balance for n at A, and $m - \frac{bn}{a}$ is the effective force at A, which multiplied by the lever a gives $ma - nb$ for the efficacioufnefs of that force in the angular velocity of the Power and weight. Now, by the principles of Mechanics, the Inertia of any bodies re-

volving

volving about a Center is as the quantities of matter into the fquares of the Brachia; and in the prefent cafe, therefore, the whole Inertia of m and n is as $m a^2 + n b^2$. Hence then, and becaufe the velocity generated in a given particle of time is as the Force directly and Inertia inverfely, we have $\frac{ma - nb}{ma^2 + nb^2}$ as the accelerating force, or the meafure of the angular velocity of the Power and weight at the end of the faid given particle of time. And I ufe the angular velocity, becaufe the arbitrary proportions in the lengths of the Brachia which may form an Equilibrium will not alter the expreffion. But again, the times of defcent by means of uniform forces, thro' a given fpace, are inverfely as the fquare roots of the accelerating forces, or meafures of the velocities generated in a given particle of time; and therefore $\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$ is a general expreffion for the time of a ftroke. This being had, the folution is eafy; for, fuppofing n only to be variable, fay as $\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$: n :: 1, a conftant or given time: $n\sqrt{\frac{ma - nb}{ma^2 + nb^2}}$ the effect in time 1, *ex hypoth.* the greateft effect which can poffibly be produced in the faid given time. Taking, then, as ufual, the Fluxion equal 0, we have, after a proper reduction, $2a^3 m^2 - 3 a^2 mnb + amnb^2 - 2n^2b^3 = 0$, and $n = \frac{am}{b}\sqrt{\frac{a}{b} + \overline{\frac{3a - b}{4b}}^2}$ $- \frac{am \times \overline{3a - b}}{4b^2}$. Therefore, in thefe forts of Engines, when the Brachia are given, the weight :

Power

Power :: $\dfrac{a}{b} \sqrt{\overline{\dfrac{a}{b} + \dfrac{3a-b}{4b}}\vert^2} - \dfrac{a \times 3a-b}{4b^2}$: 1; and if the Brachia are equal, *i. e.* if $a = b$, the weight : power :: $\sqrt{\frac{5}{4}} - \frac{1}{2}$: 1, *viz.* 0,618 : 1 nearly when the effect is a maximum. And fo, in like manner, when b, m and n are given, and a is made variable, it is eafy to fee that, inftead of the load, the beft diftance of the power from the fulcrum of the lever will be the refult of the procefs; *viz.* $a : b :: n + \sqrt{n^2 + mn} : m$. But, this by the way.

In the proportion here determined, the power m is a weight, and therefore $ma - nb$, which is the generating force, being partly employed to overcome the Inertia of the quantity of matter m, it is not wholly taken up in giving motion to the weight n; and the relative velocity is continually decreafing. But, on the other hand, if m be the force of a fpring, as is that of our atmofphere, or if n can be uniformly accelerated any how, in repeated vibrations, that there may be no fenfible diminution of the relative velocity, the whole will be exerted on the weight to be raifed; *i. e.* the tenfion of the rope or chain, by which the power is confined to act on the weight, will always be the fame as tho' the Beam were at reft; and then, by expunging ma^2 out of the expreffion for the greateft effect, $n \sqrt{\dfrac{ma - nb}{ma^2 + nb^2}}$ becomes evidently enlarged to $n \sqrt{\dfrac{ma - nb}{nb^2}}$. The confequences are thefe. 1ft, The greateft effect of this engine when m is a fpring, will always exceed the cotemporary effect where m is a weight. 2dly, The proportion of the Power and weight will then

be $n : m :: a : 2b$, as appears by taking the fluxion of $n \sqrt{\frac{ma - nb}{nb^2}} = 0$, and reducing the equation in the manner above. Whence the load to be raised for the greatest effect of a steam-engine, if the Inertia of the materials compoling its working parts be put out of the queftion, will be juft half of what is fufficient to balance the atmofphere, whether the Brachia of the lever be equal or not.

Permit me now to trouble you with two or three remarks on what I formerly laid before you concerning the proportion of the cylinders. And, 1ft, in all values of the Brachia, with regard to their lengths, and all values of n, the expreffion $\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$ for the time of a ftroke, when m is a weight, is the general expreffion to be ufed for the time. 2dly, m being confidered as a fpring, the time of a ftroke is as $\sqrt{\frac{nb^2}{ma - nb}}$; and then if, according to what I have there directed, a be taken variable, and m the reciprocal of a, the advantages to be gained by the breadth of the cylinder can only arife from a diminution of friction, and from the matter in the Beam ; for, the expreffion $\sqrt{\frac{nb^2}{ma - nb}}$ becomes conftant, and thence the ftrokes are ifochronal. I might, furthermore, proceed to examine into thefe advantages, more explicitly than is there done, upon the principles laid down, when m is a weight. But many particulars (fuch as the form of the Brachia and various appendages, with their quantities of matter and centers of gyration)

gyration) being wanting to perfect the Theory of the
conſtruction, I ſhall drop the inquiry when I have
made only one remark more. It is this. The ſhort-
neſs of the Brachia diminiſhes the reſiſtance of the
Engine to motion ; and, therefore, the inequality
which I propoſed in them was in part to avail myſelf
of that obvious advantage, without incurring the in-
convenience of enlarging the pump-bores. I ſay
it is an obvious advantage ; for, the matter in the
Brachia, that the Equilibrium may be preſerved, be-
ing inverſely as their lengths, and the reſiſtance to
motion in the direct ratio of the ſquares of thoſe
lengths, the reſiſtance of the longer arm is to that
of the ſhorter as the lengths of them directly.

Queen-ſquare, Weſtminſter,
    June 22, 1756.

II. *Obſerva-*

II. Observations on the Growth of Trees: By Robert Marsham, of Stratton in Norfolk, Esq; communicated by the Rev. Steph. Hales, D. D. F. R. S.

Measures of Trees, taken in April 1743, before they began to shoot; and again in Autumn 1758, after the Year's Growth was completed. The Measure taken at 5 Feet from the Earth.

FIRST TABLE.

Read Jan. 11, 1759.

	Circumf. in Spring 1743.			Circumf. in Autumn 1758.			Increase in 16 years.			Content in 1743.			Content in 1758.			Solid increase in 16 years.		
	Feet	Inches.	8ths of In.	Feet	Inches.	8ths of In.	Feet.	Inches.	8ths of In	Cubicfeet.	Quarters.	Inches.	Cubicfeet.	Quarters.	Inches.	Cubicfeet.	Quarters.	Inches.
1. Ash, planted since 1647	9	10	4	11	1	0	1	2	4	60	1	318	76	3	43	16	1	157
2. Oak, past thriving, but found	9	4	4	10	1	0	0	8	4	54	1	336	63	2	80	9	0	176
3. Oak, about 80 years old	6	3	3	7	8	3	1	5	0	24	0	284	36	1	408	12	0	124
4. Scotch Fir, feed in 1698	5	4	6	6	6	0	1	1	2	17	3	48	26	1	270	8	2	222
5. Oak, planted above 60 years	5	11	1	7	7	3	1	3	2	21	3	224	32	0	174	10	0	382
6. Spanish Chesnut, near 60 years old	4	4	0	5	6	3	1	2	3	11	2	408	18	3	270	7	0	294
7. Another, 45 or 46 years old	2	9	6	4	4	4	1	6	6	4	2	391	11	2	408	7	0	17
8. Oak, planted by me in 1720	2	11	2	5	1	2	2	2	0	5	1	116	16	0	260	10	3	144
9. Scotch Fir, planted 1734, 2 feet high	1	11	6	4	0	0	2	0	0	2	2		10	0	0	7	2	0
10. Pinaster, planted in 1734 or 1735	2	5	1	4	3	1	1	10	0	3	2	260		1	68	7	2	240
11. Oak, set an acorn in spring 1719	1	7	0	2	8	2	1	1	2	1	2	116	4	1·396		2	3	220
12. Oak, planted in 1720 or 1721	2	9	5	4	9	4	1	11	7	4	2	391	14	0	170	9	1	217
										212	0	200	322	0	333	109	0	33

No.

Now as the twelve trees above, con ained 213 cubic feet 300 inches of timber in spring 1743, and have increafed to 322 cubic feet 333 inches in autumn 1758; that is, 109 cubic feet 33 inches in 16 years growth; if all the trees were of the fame kind, 109 feet pays 3 *per cent.* for ftanding: and the fix oaks pay near the fame intereft, although one of them, N° 2. appeared paft thriving in 1743; for the increafe of the fix oaks is from 112 feet 1 quarter 171 inches of timber, to 167 feet 138 inches, *i. e.* 54 feet 2 quarters 399 inches; which is above 3 *per cent.* But if you take only the five thriving oaks, then their content is, from 57 feet 3 quarters 267 inches, to 103 feet 2 quarters 58 inches; *i. e.* 45 feet 2 quarters 223 inches of timber; or near 5 *per cent.* And the increafe of the moft thriving oak, N° 8. appears, by the above table, to pay above 12 ½ *per cent.* and the Scotch fir, N° 9. being under 2 feet and half of timber in fpring 1743, and 10 feet in autumn 1758, pays above 18 ¾ *per cent.* Befides it fhould be confidered, altho' I meafured the largeft and moft thriving oak and Scotch fir in 1743, yet feveral others of the fame age, both oaks and Scotch firs, have greatly exceeded the meafured trees for many years paft; *e. g.* the oak N° 11. appears by the table two feet 8 inches 2-8ths in circumference; and another juft by it is 2 feet 11 inches 6-8ths; and an oak tranfplanted from this grove, is 3 feet 9 inches 5-8ths round; yet this laft tree was confiderably lefs than the firft when removed, and not planted in a better foil, and yet is 1 foot 1 inch 3-8ths larger than the original tree. The firft contains 4 feet 1 quarter 336 inches, and has gained 2 feet 3 quarters 220 inches

inches in fixteen years: the laft contains 8 feet 3 quarters 68 inches; and, fuppofing them equal in 1743, gains 7 fee, 384 inches; *i. e.* above 2 ¼ the increafe of the firft tree. But notwithftanding the tranfplanted oak is thus much larger than the original oaks in the grove, yet as the tranfplanted tree does not run half the height of the trees in the grove before it heads, they differ but little in their quantity of timber.

The following table fhews the monthly increafe of trees in the years 1757 and 1758. As I endeavoured to take the meafures with as much exactnefs as was in my power, I cut three, four, or more notches in the bark of each tree, that my line might always be confined exactly to the fame place. I obferved, if I meafured foon after a rain, whilft the bark was faturate with water, the tree would be ⅛ of an inch larger than after a day or two of dry weather. I may here add, that all the meafures of circumferences of trees are taken at 5 feet from the earth: and confequently the folid meafures muft include 10 feet in length. I generally made ufe of Keay's Tables in the folid meafures, which go no lower than quarters of inches in girts: which is not fo exact as it ought to be.

Measures of the monthly Increase of the most thriving Trees I had, of the following several Kinds, taken at 5 Feet from the Ground in the Years 1757 and 1758.

TABLE II.

Column date-notes (measurement periods):

- **1 June 1757.** — Most of these trees were 1-8th of an inch less in the beginning of April 1757, than at their last measure; and had not increased on the 1st of May; but on the 1st of June flood thus.
- **3 July 1757.** — July 3, 1757, the last month was very dry.
- **3 Aug. 1757.** — August 3, 1757, the first half of the last month was absolutely dry and hot, and the last half frequent flowers.
- **4 Sept. 1757.** — September 4, 1757, from 3d Aug. to 10th very hot; the rest of the last month much cooler; and the last half half frequent flowers.
- **Nov. 1757.** — November.
- **1 June 1758.** — June 1st, 1758, very dry spring.
- **3c June 1758.** — June 30, 1758, last month rather dry.
- **31 July 1758.** — July 31, 1758, last month very rainy, but hot.
- **29 Aug. 1758.** — August 29, 1758, last month hot, and rather moist.
- **29 Sept 1758.** — Sept. 29, last month very wet and cool.

The baseline measure column is headed **Feet | Inches | 8ths of In**, and was measured October 6, 1756; and on the 6th of November they were rather less.

Trees measured (each value given as Feet · Inches · 8ths):

Tree	Base (Oct 1756)	1 June 1757	3 July 1757	3 Aug. 1757	4 Sept. 1757	Nov. 1757	1 June 1758	30 June 1758	31 July 1758	29 Aug. 1758	29 Sept. 1758
1. An Oak planted in 1720, No. 8. in the other table	4·9·7	4·10·1	4·10·3	4·10·7	4·11·2	4·11·2	4·11·3	4·11·6	4·11·6	5·1·2	5·1·7
2. A Beech, a seed in 1741	0·11·0	0·11·1	0·11·5	1·0·1	1·0·2	1·0·0	1·0·1	1·0·3	1·1·7	1·2·2	1·2·5
3. A Scoth Fir, planted in 1734, about 2 feet high	4·2·4	4·3·0	4·3·1	4·3·2	4·3·4	4·3·6	4·4·4	4·4·0	4·4·4	4·6·0	4·6·0
4. An Oak, an acorn in spring 1719, never removed	2·9·4	2·9·2	2·9·6	2·10·1	2·10·2	2·10·2	2·10·4	2·10·6	2·11·1	2·11·4	2·11·6
5. A Spanish Chesnut 45 or 46 years old, No. 7. in the other table	4·2·5	4·2·4	4·4·4	4·3·1	4·4·4	4·3·4	4·3·0	4·3·5	4·4·1	4·4·4	4·4·4
6. An Elm, about 25 years old	1·7·0	1·7·1	1·8·0	1·8·2	1·8·6	1·8·6	1·9·0	1·9·9	1·9·7	1·10·8	1·10·8
7. A Spruce Fir, planted in 1735, about 2 feet high											
8. A Larch, planted in 1749	1·0·7	1·5·1	2·7·1	5·2·2	7·2·3	3·6·2·10	3·6·2·10	4·6·7·3·0	4·6·7·3·0	5·4·0	5·4·0
9. A Willow, set in spring 1736											
10. A Beech, a seed in 1733											

TABLE

TABLE III. *Shews the Increase in Circumference, and in solid Measure, of each Tree in* 1758.

	Circumfer. in 1757.			Circumfer. in 1758.			Content in 1757.			Content in 1758.			The year's solid Increase of 1758.			Interest the Trees pay for standing.	
	Feet.	Inches.	8ths of In.	Feet.	Inches.	8ths.	Cubic feet.	Quarters.	Inches.	Cubic feet.	Quarters.	Inches.	Cubic feet.	Quarters.	Inches.		Per Cent.
1. The Oak	4	11	2	5	1	2	15	0	188	16	0	260	1	0	72	or near	7
2. The Beech	1	0	4	1	2	3	0	2	216	0	3	174	0	0	390	above	36
3. The Scotch Fir	4	3	6	4	4	6	11	1	68	11	2	408	0	1	340	about	4
A Scotch Fir, not in the 2d Table, planted in 1735	4	1	4	4	4	0	10	1	296	11	2	408	1	1	112	above	12½
4. The Oak	2	10	2	2	11	6	5	0	30	5	2	216	0	2	186	above	12⅛
5. The Spanish Chesnut	4	3	4	4	4	4	11	1	68	11	2	408	0	1	340	near	4
6. The Elm	1	8	4	1	10	3	1	2	408	2	0	174	0	1	198	near	21
7. The Spruce Fir	3	6	2	3	8	0	7	2	270	8	1	264	0	2	426	above	9¾
8. The Larch	1	3		1	5	1	0	3	392	0	1	8	0	1	48	above	28¼
9. The Willow	5	2	3	5	4	3	16	2	318	17	3	48	0	0	162	above	6½
10. The Beech	2	10	4	2	0	5	2	0	174	2	3	0	0	1	258	above	19
Weymouth Pine	1	6	2	1	9	0	1	1	270	1	3	▵84	0	2	14	about	36

N. B. I measure the oak Nº 4. as three feet round, as it wants only 1-4th of an inch of that ... ; and the Weymouth pine as 1 foot and 6 inches, tho' it is 1-4th of an inch more.

As the Scotch fir, Nº 3. has been sickly for two years past; therefore I add another Scotch fir (one year younger) to shew the growth of that kind of tree; and the extraordinary increase of the Weymouth pine induced me to put that in also, tho' I had not measured it monthly.

The

The great Lord Bacon fays, " *the improvement of the ground* " *is the moſt natural way of obtaining riches.*" What great fortunes might be raiſed, by thoſe that have property, in the vaſt heaths and downs, or fields of poor land, in this kingdom, by planting parts of them? which would alſo add great beauty to the country, and render the dwelling much more comfortable to the neighbourhood, by the ſhade in ſummer, and warmth in winter. Some parts of theſe great waſtes would produce good oak; and where the ſoil is moiſt, poplar, alder, and other aquatics, would be very profitable to the planter. The chalky ſoil ſeems the leaſt promiſing; yet beeches ſometimes thrive well upon it. The fir kind, eſpecially the Scotch fir, will grow ſurpriſingly upon poor ſandy land; but woods of fir ſhould be guarded with an out-line of birch and beech, to break the force of ſtrong winds. Birch, being the quickeſt grower, will beſt protect the young fir; but as birch, after a few years, is eaſily blown down, ſo beech will be wanted to defend the firs as they become large: for I have ſeen broad glades made by the wind through great woods of fir in Switzerland : which, perhaps, might have been prevented, at leaſt in part, by an out-line of beech.

I know ſome think, that poor land cannot produce large trees; yet the oak at Northall in Hertfordſhire, whoſe beautiful head ſpreads a circle of above 40 yards diameter, ſtands on a dry and deep ſand ; and the fine cheſnuts and beeches by Mr. Naylor's grand caſtle of Herſt Monceux in Suſſex, grow in a light ſandy ſoil : and I have found, by experience, the Weymouth, Scotch, ſpruce, and ſilver firs, which I planted in a poor ſandy ſoil, are larger and finer trees, than others ſet at the ſame time in much better land. Perhaps it may require a rich clay to produce ſuch trees as the noble grove of oak in the Earl of Powis's park by Ludlow, or Lord Ducie's vaſt cheſnut at Tortworth, in Glouceſterſhire, which I meaſured 46½ feet in circumference at near 6 feet from the ground.

Although theſe ſlight obſervations are not ſo deſerving the attention of the Royal Society as I could wiſh ; yet they may poſſibly be the means of producing better ; and for my own part, I ſhall always eſteem it a great honour that they were communicated by Dr. Hales.

R. Marſham.

Stratton, Dec. 18, 1758.

Fig 1
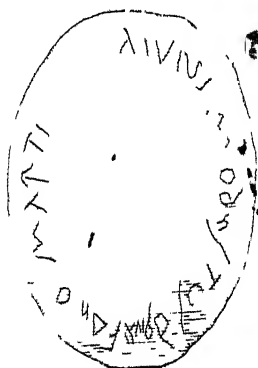
Fig 2.

Fig 3

Fig 4.

Fig 5.

Fig. 7.

Fig. 6.

A
152 ft
B

C

Fig. 8.

The Fort at Bossens in the parish of St Erth Cornwall.

136 ft

6   12   18
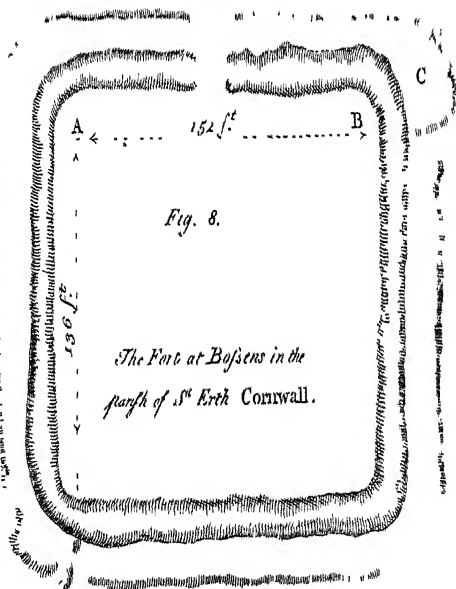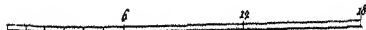
A Scale of Inches from Figure 2 to 7.

J Mynde f:

III. *An Account of some Antiquities found in* Cornwall : *In a Letter from the Rev.* William Borlase, *M. A. F. R. S. to the Rev.* Charles Lyttelton, *L. L. D. Dean of* Exeter.

Rev. Sir,   Ludgvan, Dec. 4. 1758.

Read Jan. 11, 1759.   **A**S I know the pleasure, which every branch of ancient literature gives you, I should not excuse myself, if I did not communicate to you a late difcovery of Roman antiquities in thefe wefternmoft parts of Britain.

In the year 1756 a farmer at Boffens, in the parifh of St. Erth, driving his oxen from the field, perceived the foot of one of them to fink a little deeper than ordinary into the earth at A, *fig.* 8. *(See* TAB. I.) Curiofity, and the hopes of treafure, led him foon after to fearch the place; where was foon difcovered. a perpendicular pit, circular, of two feet and half diameter. Digging to the depth of 18 feet, there was found a Roman patera *(fig.* 1. & 2.) : about 6 feet deeper, the jug, *fig.* 3 : near by, among the rubbifh, the ftone, *fig.* 4; a fmall millftone, about 18 inches diameter : then another patera, with two handles, in other particulars of the fhape and fize as *fig.* 2. but unfortunately miflaid, and not now to be found. Intermixed with thefe were found fragments of horns, bones of feveral fizes, half-burnt fticks, and many pieces of leather, feemingly fhreds of worn-out fhoes. Having funk to the depth of 36 feet, they found the bottom

bottom of the pit concave, like that of a difh or bowl. There was a fenfible moifture, and moftly wet clay, in all parts of the pit. On each fide there were holes at due diftances, capable of admitting a human foot, by which perfons might defcend and afcend. There is no doubt but this work muft have been intended for a well: but a pit fo deep, and of fuch narrow dimenfions, muft have been funk thro' a ftony ground with much difficulty, and with tools very different from thofe now in ufe.

Coming to the fpot on the 22d of May laft with Henry Davies, Efq; proprietor of the land, who firft favoured me with the notice of this difcovery, I found, on the higher part of the tenement, in a field called the Rounds, the remains of a fort : the length of it, bearing nearly north and fouth, was 152 feet; the breadth, from eaft to weft, about 136 feet. The fofs on the outfide is ftill difcoverable; the walls difmantled, but fufficient remains to fhew, that the work was rectilinear, with the angles rounded off; a manner of fortifying, which the Romans were generally fond of, as may be feen by their ftations *per lineam valli* (Horfley *Britannia Romana*, p. 113, and many other places). At the north corner, B, there was an additional building, projecting outwards beyond the rampart, about 30 feet long, not quite fo wide: at the fouth angle at D there are the figns of a building of like kind : thefe were the proceftria of the fort. The fhape and fize of the work, as it ftands at prefent, may be feen in the drawings annexed, *fig.* 8.

Upon examining the rubbifh near the pit, I found the cut ftone, *fig.* 5. part of a large ftone vafe, and

part

part of an earthen fepulchral urn : I found alfo fome fragments of leather : all which, with what was found before, and brought me, I fhall beg leave to ·defcribe, with a few obfervations.

Fig. 1. and 2. are two views of the patera : it was made of tin, the 20th of an inch thick, four inches and a half wide at the brim, but growing narrower downwards, was at the bottom, which was flat, two inches and a half in diameter. The bottom of the infide is reprefented, *fig.* 1. in its real fize. *Fig.* 2. is the fide of the fame patera, by the fcale annexed. The Roman patera was not always of the fame dimenfions. When it was of the larger fize, its ufe is well known to have been for receiving the blood of the victim, or to be carried before the priefts with other offerings ; but when of fmaller dimenfions (as this is), either to offer libations of water, oil, or wine, on the altar (whence on medals the hand fo often ftretched out, holding the patera towards the altar), or to participate the rites of facrifice by drinking. This patera had no anfa, or handle ; tho' that which is loft, and has been mentioned above, had one on each fide : and indeed thofe found in England generally have. Mr. Addifon obferves [*Travels*, of his works, p. 115.], that it is not fo common to find paterae with handles to them abroad ; but that a patera without a handle would be as fingular here [in England] as one with it at Rome : and Mr. Horfley, (p. 191), that all paterae, which he had feen upon any altars in Britain, had handles, tho' of different fizes and fhapes : but it may be obferved, that the five paterae, which I have feen found in Cornwall, never had any, and are therefore the more remarkable. It

is more rare ftill to find thefe feemingly trifling cups
and difhes infcribed to a particular deity; but moft
uncommon to fee them diftinguifhed by the names
of the donor and his father, as well as the name of
the deity to which they were dedicated. This patera,
found at Boffens, about three miles north-eaft of St.
Michael's Mount, is a fingular inftance of the latter
ufage, and has the following infcription engraved on
its bottom, in fhape, and fize, and circular line, as
in *fig*. 1.

λιVιυς Μodη/τυ/ dqιvλι Γ Δ ηο ΜΑΤΤι

Which I read thus, till better information. *Livius*
Modeftus Driuli (or *Douiuli*) *f.* (for *filius*) *Deo*
Marti.
  The two firft words are very plain (tho', like the
whole, a mixture of Greek and Roman characters),
and not rare in Roman hiftory. *Livius* is too well
known to need any comment; and as it is well
known, that the virtues oftentimes gave names to
perfons, it is as certain, that modefty among the
reft had that privilege. Sometimes the perfon, who
had this name, was called Verecundus [*Diis Manibus*
Verecundi, in a Roman monument at Skirway in
Scotland; *Horfley*, 199: and the fame name is to be
traced in another monument, *Ibid*. Plate 64. N°. X.]
Here it is *Modeftus*, of which we have alfo inftances,
as *Publius Ælius Modeftus Præfectus*, who dedicated
an altar to Hercules, *Ibid*. Pl. 16. N°. XLI.; and
Caius Murrius Modeftus miles in Somerfetfhire, *Ibid.*
Pl. 71. N°. II.

The

The letters in general of this inscription are badly shaped; but in the third word particularly the characters are more perplexed than in the rest. The first letter is the Greeian small *delta*; the second I take to be the little *ro* of the Greeks reversed (*viz.* with the long stroke on the right, instead of its being as usual on the left hand): the other letters are more truly delineated, tho' somewhat crowded: so that I take this word to be *Driuli*, a name, which I do not recollect to have met with before: but if the second letter shall appear to the learned to be more likely intended for the Greek character ꞵ, or *ou*, this word will then be *Douiuli*, or *Duilii*, a name very honourable among the Romans. *F* stands for *filius*, as usual; and the two last words are beyond doubt *Deo Marti*. The language is Roman; but the λ, · δ, △, η (which in the words *Modestus* and *Deo* is used for the Latin *e*), and the *ro*, or *ϙ*, are proper to the Grecian alphabet. The *R* in *Marti* is singular, intended for P, the Greek capital *Ro*; but instead of the semicircular part joined to the upright, thro' the incorrectness of the engraver, it has a demi-hexagon, like a canopy, over the upright line. The *o* is oval, not round as with the Latins; and the A has no transverse stroke. The other letters are common to the Greeks and Romans.

That this Latin inscription should have so much of the Greek character is remarkable. It is well known, that the Druids used the Greek letters. Whether the person, who consecrated this patera to Mars, might intrust the engraving to one of the Druid sect, or whether the engraver was one of the auxiliary cohorts and natives of Greece, as the Thracian and

D   Dalmatian

Dalmatian horse (for such we certainly had in Britain), it is in vain to inquire; but neither of these conjectures is improbable. There are, I think, but two inscriptions in the Greek language as yet found in. Britain; but in the Latin language this seems to me the only one as yet discovered in the island written in Greek characters.

Fig. 3. is a jug or jar (of tin also) containing four quarts one pint and $\frac{3}{4}$ of a quartern, wine measure: its weight 7 ℔ 9 $\frac{1}{2}$ *oz.* It is the præfericulum of antiquarians, a vessel used to bring the holy water, or other sacred liquor, to the altar. It seems to have had that name from its being carried in procession before the priests in a kind of shallow bason (which Festus chuses rather to call the præfericulum, as see Montfaucon *de pateris,* tom. ii.), in much the same manner as the bason and ewer were formerly used among us.

Fig. 4. & 5. are of stone. The first and largest weighs 14 ℔ 1 *oz.* (avoirdupois) and 11 *dwts.* amounting, if I mistake not, to 18 ℔ Roman and 337 grains. The second and smaller stone weighs 4 ℔ 1 *oz.* (avoirdupois) and 7 *dwts.* or 5 ℔ $\frac{1}{4}$ Roman and 95 grains. By the holes these stones have near the top, they were probably designed as weights, whereby provisions were bought for, and afterwards shared among, the soldiers of the fort.

The ancients sometimes made their weights of stone, and of different shapes, round, rectangular, and conical: sometimes they were of marble, as those exhibited by Gruter, p. 221, 222. (as Kemp quotes him), and that in the *Monumenta Kempiana,* p. 152. and in other repositories. These here are
both

both of the dove-coloured Cornifh granite, difcoloured as it feems by fire. But the ancients feem not to have made their weights of ftone by choice, but for want of metal; for they could not be ignorant, that lead, brafs, or iron, was more compact, ponderous, and durable; and that ftone was liable to become heavier, by affuming into its fubftance the moifture and weight of adjacent bodies, and on the other hand to be corroded, and become more porous and lighter, by means of any penetrating acid, or heat or drought; and fo either exceed or fall fhort of the intended ftandard. Thefe inconveniences of ftone weights the ancients, I fay, could not but forefee; they are alterations which the materials could hardly efcape: and therefore I conjecture, that the weights before us have varied: they both exceed the number of Roman pounds, which they were probably at firft adjufted to. The great weight indeed has been increafed but a fmall matter, *viz.* 337 grains more than 18 pounds; the fmall one more in proportion, *viz.* 1406 grains more than 5 pounds, *i.e.* above a quarter of a pound: nor is it to be wondered at, that, tho' both of the fame fort of ftone, they fhould have acquired different quantities of weight; for the addition muft have been according to the nature of the rubbifh in which they have lain. If there were any metallic ores (iron, for inftance, copper, or tin) or impregnated waters, in the rubbifh, where thefe ftones were depofited, then the addition would be great, otherwife the addition would be no more than that of common moifture or earth.

Fig. 6. is part of a vafe or bowl, fometimes made of brafs, or richer metal, but here of ftone. This,

I ap-

I apprehend, is what Feftus calls the præfericulum [*Præfericulum, inquit Feftus, vas æneum, fine anſâ, appellatum patens ſummum, ut pelvis, quo ad ſacrificia utebantur in Sacrario.* Montfaucon, tom. ii p. 142.); and by its gradually increafing thicknefs towards the bottom, appears to have been of like defign with that exhibited in the *Antiquities of Cornwall,* plate xxi. fig. 5. p. 274. This vafe was of curious grey granite, formed by turning, well polifhed within, fomewhat difcoloured without, as if it had fuffered by fire.

The fmall millftone, by the fmoothnefs of one fide, fhews that it had been much ufed; and was fuch, without any material difference, as is now ufed in the iflands of Scilly (and elfewhere) for hand-mills to grind corn in times of fiege and confinement, and muft be abfolutely neceffary in all forts.

The bones and horns may be fuppofed to have belonged to animals, either facrificed, or killed for the fuftenance of the garrifon : the afhes, and half-burnt fticks, are the remains of facred or culinary fires. The fragments of leather are for the moft part patched, and coarfely fewn together ; but one piece, which I found more intire, may contribute perhaps to fhew us the fhape of the Roman calceus of thofe times ; and may be feen *fig.* 7. by the fame fcale with the reft. Some bits of leather were alfo pierced with circular holes ; but whether parts of the calceus, cothurnus, or any border for the habit, armour, or vehicle of the officers, enough does not remain to decide.

I fhall make no other reflection at prefent on thefe antiquities, than that the infcription is the firft difco-
<div align="right">vered</div>

vered in this county of fuch high antiquity; and will fatisfy the learned, that the Romans had penetrated into the wefternmoft parts of Cornwall before the empire became Chriftian : that the facrifical veffels, the pateræ, and præfericulum, are of tin, the natural product of Cornwall : the vafe, the weights, the millftone, are alfo of Cornifh granite : and by the walls, the religious utenfils, the weights, the quantity of fhoes, bones, horns, vafes, urn, and afhes, this fort appears to have been that of a fixed garrifon, not a temporary occafional fortification : that by the fhape of this fort, and the antiquities difcovered in it, it was a Roman fort.

I remain,

S I R,

Your moft obedient Servant,

William Borlafe.

IV. *A new improved* Silk-Reel. *By the Rev.* Samuel Pullein, *M. A.*

Read Feb. 1, 1759.

THE following paper, it is hoped, will help to promote the culture of filk in our American colonies, and to bring it to that perfection, which at prefent is fcarce found in any country, but Piedmont.

When filk is reeled from the cocoons, the thread is fmeared with the natural gum of the filk foftened by the heat of the water out of which it is reeled.

If

If the feveral rounds of this thread, as it falls on the reel, touch one another in their whole extent, it is then fo glued together, as to be intirely ufelefs for any perfection of manufacture, it being impoffible to wind it off without tearing and breaking. This fault in the filk countries goes under the technical name of the Vitrage ; and there are many degrees of it, according to the caufe by which it is produced.

From the introduction of filk into Europe to this day, the preventing of the Vitrage has employed fome of the moft knowing men in the countries where filk is produced.

To change the pofition of the filk thread, that it might not always fall on the fame part of the reel, the guideftick was introduced. This received a progreffive and regreffive motion, by means of two wheels, the one fixed to the axle of the reel, and communicating its motion to the other by means of a band. To make thefe wheels perfect, certain determined and precife proportions are to be obferved in the diameters of their grooves, otherways, notwith-ftanding the motion of the guideftick, there may be a total Vitrage of the whole hank of filk ; and, even when the proportion of the wheels is perfect, there may be reckoned up thirteen or fourteen accidents, any of which happening will caufe a Vitrage : but tho' thefe inconveniencies attend all thofe reels which ufe a band, yet, becaufe the ordinary fort of them are eafily made, they are ftill ufed by the common people in moft filk countries.

No nation has brought its filk to a greater perfec-tion than the Piedmontefe : but it was only by flow degrees, and in a long tract of time, that they ar-
rived

rived at this. They firſt ſuppreſſed the old method of reeling the ſilk over a bobin, which was found to give it a flat form ; and they ſubſtituted in its ſtead that excellent method of the croiſſure, which renders it round and compact. They then applied themſelves to correct the imperfections of the guideſtick, and to eſtabliſh proper proportions between the wheels which gave it motion : but, notwithſtanding theſe and other improvements, they ſtill found, that, ſo long as theſe wheels were turned by means of a band, they could not arrive at the perfection neceſſary to make ſilk fit for organcine or warp, becauſe the inaccurate motion of the band made the moſt juſt proportion which they could eſtabliſh between the wheels quite ineffectual, and conſtantly produced a Vitrage.

It was on this account that they totally ſuppreſſed the uſage of the band, and ſubſtituted in its ſtead four wheels with a determined number of teeth, whoſe revolutions, being uniform, gave the guideſtick a proper motion for preventing the Vitrage. This, and ſome other regulations, are eſtabliſhed by laws, which are rigorouſly put in execution, leſt the common people ſhould thro' indolence relapſe into their old cuſtoms. By adhering to this reel, the Piedmonteſe are able to give that perfection to their raw ſilk, which fits it to be thrown into organcine or warp ; and to raiſe the value of each pound of ſilk reeled in this manner to one third more than it would have if reeled otherways.

The French were deſirous of making raw ſilk fit for organcine or warp among themſelves, which hitherto they have had from Piedmont. Whether they
thought

thought the Piedmontefe reel could not eafily be brought into common ufe, or whether a little vanity hindered them from copying or improving the inventions of other nations, I will not determine. However that be, they applied themfelves wholly to the improvement of the band-reel.

Two perfons, *viz*. M. St. Prieft of Languedoc, and M. Vaucançon of the Academy of Sciences at Paris, have, within thefe three or four laft years, brought the band-reel to the greateft perfection of which it is capable. Either of their reels will make raw filk fit for organcine or warp, provided they are accurately made ; but a fmall error in their conftruction deftroys their perfection. I have now in my poffeffion a reel made according to M. Vaucançon's method; where I can fhew, that in two wheels, whofe diameters differ but very inconfiderably, one fhall reel the filk properly, and the other throw it into a total Vitrage. I have however given a defcription and plate of this reel, in a treatife on the culture of filk, which I publifhed laft year, becaufe its contrivance is more fimple than that of M. St. Prieft, and performs as well.

The Piedmontefe reel is free from the inconveniencies of all thofe reels which ufe a band : the maker cannot eafily err in the conftruction; the weather cannot affect its operation ; nor is it fubject, during its work, to the many irregularities of the band-reel. The chief objections which have hindered its being commonly ufed are, that four toothed wheels are more difficult to be made than two plain grooved ones ; and that being made only of wood, they are eafily broken. So that it is neceffary to

have

have a double fet of them, in order to prevent thofe
delays which might happen by their breaking; be-
caufe any delay in the reeling of filk occafions a con-
fiderable detriment.

Our happier conftitution doth not admit fuch
rigorous laws as that of Piedmont. To make the
common people adopt any new contrivance, they
muft not only know that it is the beft, but they muft
alfo feel that it can be eafily practifed. Thofe who
fuperintend public filatures in our colonies may in-
deed, for the prefent, keep their workmen to the
Piedmontefe reel; but public filatures can no more
produce large quantities of filk than public fpinning-
houfes could produce the vaft quantities of linen-
yarn which are now raifed from private family-
wheels: And when the management of filk-worms
becomes more common in our colonies, and people
find that, by reeling their own cocoons, their profit
will be nearly doubled, and this with much lefs
labour than was ufed in rearing the worms ; when
this, I fay, fhall happen (and it is to be hoped it foon
will), then the making of the Piedmontefe reel more
fimple, and more familiar to the common people,
will I believe appear a thing of confiderable import-
ance to our filk manufactures.

I fome time ago turned my thoughts to the effect-
ing of this; and fhould before now have put them
in execution, had I not diftrufted my own abilities to
perform a thing, which the Piedmontefe themfelves
had not attempted. I have now conftructed a reel,
which, without any additional mechanifm, performs
the work of the Piedmontefe reel, and ufes but two
wheels with teeth inftead of four wheels. It has

been examined by fome gentlemen in the filk-trade, who are well acquainted with the reels of that country, and allowed to work with much more fimplicity, and greater perfection. Nor can I account why this improvement was not before attempted, but from the ftrong biafs of habit confining the poweis of invention.

Some advantages,which this reel feems to have above the prefent Piedmontefe reel, are, 1ft, that by a new proportion ftruck between the wheels, the filk thread will not be laid in the fame place on the reel till after the winding of near fix hundred yards: by which means, the thread having time to dry, all Vitrage is avoided; whereas thofe now ufed do not reel above an hundred and fifty yards till the thread is laid in the fame place.

2dly, I have added to this reel an improved method of making the double croiffure in as eafy a manner as the Piedmontefe now make the fingle one; the double croiffure giving a confiderable perfection to the filk above what it obtains from the fingle one.

As the limits to which I purpofe confining this memoir will not allow of a more extended defcription, which, to gentlemen not verfed in thefe matters, may already be thought too long, I fhall avoid a greater trefpafs on your attention, by a brief reference to the annexed figure [*See* TAB. II.], and to the model before you [*See* TAB. III.].

Tab. II. reprefents the prefent Piedmontefe reel. The pofition of the four wheels is fhewn at A, B, C, and D: the toothed wheel A is fixed upon the axle

of

Fig. 1
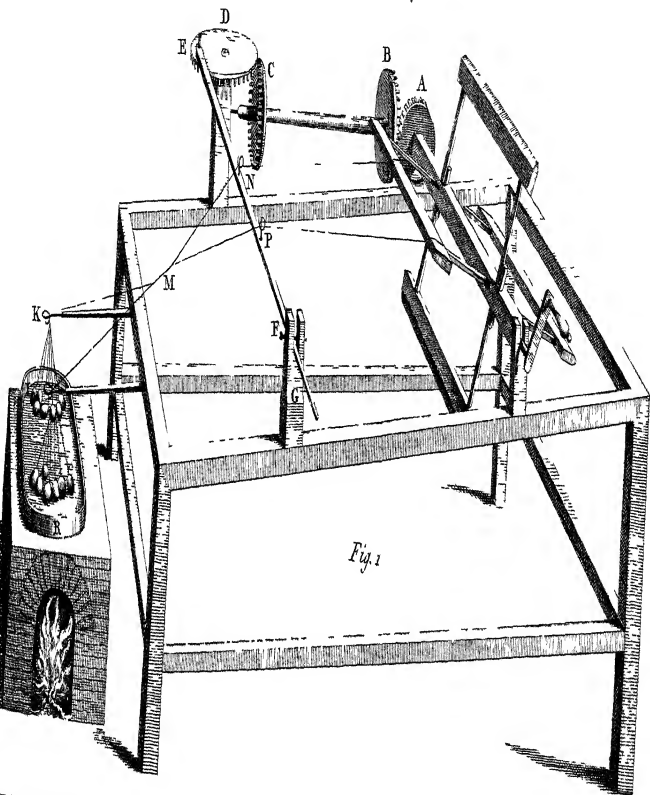
of the reel ; and, as the reel turns, it gives motion
to the two wheels B and C, which are fixed upon
one common arbre. The wheel C moves the wheel
D, which is placed horizontal; and this, as it re-
volves, makes the guideſtick E F play forward and
backward in a groove made in the upright G, the
guideſtick playing freely on a pin E fixed in this
wheel. Two threads of ſilk, drawn from two par-
cels of cocoons, which lie in the copper of hot
water R, are paſſed thro' the two loops of the ſtop-
wires K and L : they then are twiſted round each
other at M, which is the ſingle croiſſure of the Pied-
montefe. At M they are again ſeparated, and paſs
each through its own guide-wire at N and P ; and
from theſe they go to the reel, where, as the reel
turns, the motion of the guideſtick continually varies
their poſition, and hinders them to be laid on the
ſame place.

The model before you, repreſented by *Tab.* III. ex-
hibits the new conſtruction which I have given to this
reel. The two wheels C and D of *Tab.* II. are laid
aſide as ſuperfluous : the arbre Q paſſes thro' a nich in
an upright ſupporter R, and, by a winch at its end A,
gives motion to the guideſtick. The plane of its
motion in the Piedmontefe reel is horizontal, but
here it is perpendicular. This perhaps made them
think that the effect would not be the ſame; but it
doth not cauſe any eſſential difference ; and if it had,
there was a moſt eaſy remedy for it.

Here therefore the expence and trouble of making
and adjuſting two wheels and two ſets of teeth are
ſaved, the hazard of breaking and going out of order
leſſened, and the machine made more ſimple, and
more familiar to the underſtanding.

As

As I intend, at a convenient time, to give a more ample account of the method in which filk is reeled, and of the various attempts which have been made to improve the filk-reel, together with their fuccefs, that perfons may not wafte their time upon fruitlefs experiments ; and having hopes alfo that I fhall be able to add fome further improvements to it, of which I have not as yet had leifure to make trial ; for thefe reafons I fhall for the prefent take leave of this fubject, at leaft with this pleafing thought, that the pains I have taken may poffibly hereafter promote the induftry and profit of fuch poor families as are, in all countries, the fupporters of the filk manufacture.

Samuel Pullein.

In *Tab*. III. *fig*. 1. at A is fhewn the manner in which I have made a winch D, fixed in the arbre Q A, anfwer the fame purpofe as the two wheels with teeth, which are ufed in the Piedmontefe reel ; by which the mechanifm is rendered more fimple, and the guideftick F moved to the fame advantage as with the wheels.

At *fig*. 2. is fhewn the double croiffure, made by means of the crofs B, which I fhall call a fwivelcrofs. It is made of very flight fcantling, fuch as a common lath may afford. At *b b* are fixed two little ivory or brafs wheels with fmooth acute-angled grooves in them to receive the threads which come from the two ftop-wires *b b*. One end of the fwivelcrofs K is fomewhat broad, and refts upon the guideftick F directly in the middle between the two guide-wires *c c*. Here it moves and plays freely

on

Fig. 2.

c

n

R

m

F

c

Fig. 1.

J. Mynde sc.
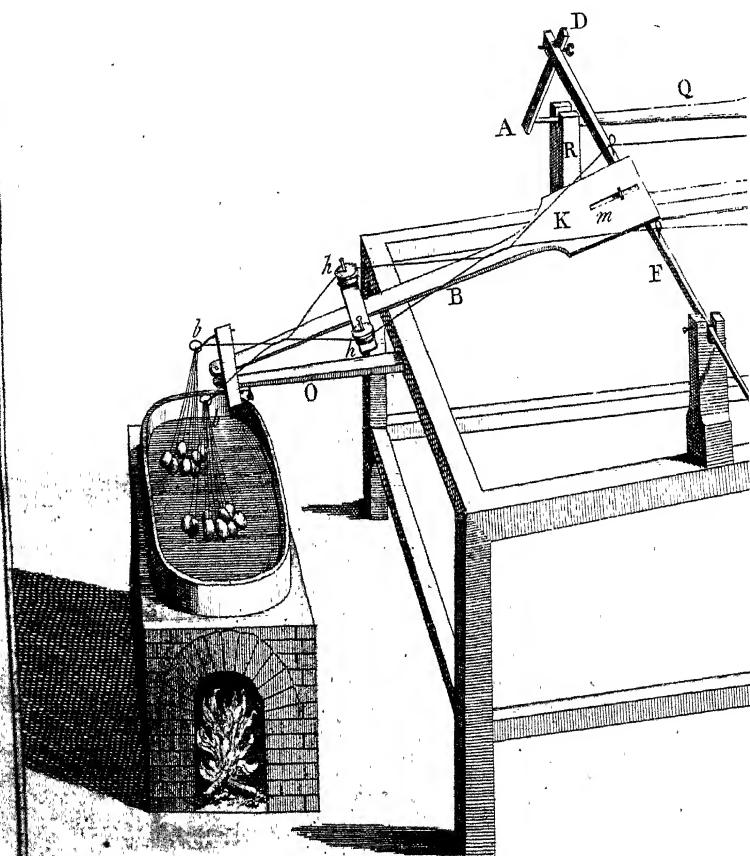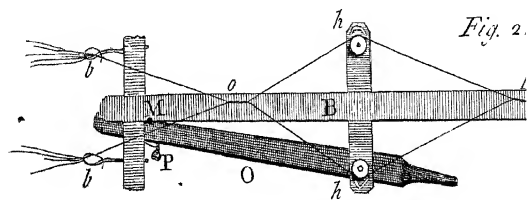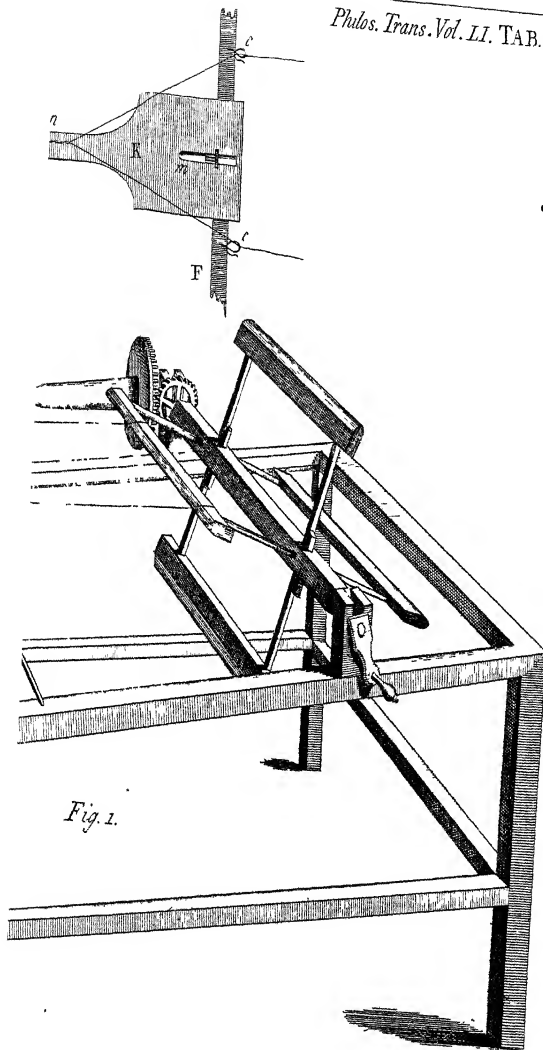
on a pin fixed in the guideftick, the pin paffing thro' the nich *m*. The other end of the fwivel M contains the ftop-wires *b b*, and is attached to the end of the •rod O, which projects from the front bench of the reel. This attachment is made by means of a piece of packthread with a knot on its end ; which packthread paffes thro' a hole in the fwivel and in the piece O, and being fixed to a peg P, which turns in the under part of the piece O, is drawn to a proper degree of tenfion to allow the fwivel to obey all the motions of the guideftick.

The manner of making the double croiffure is thus : The two filk threads are paffed thro' the two ftop-wires *b b*, and thro' the two guide-wires *c c*, and fo are faftened to the reel. Then either of the two threads is taken in that part of it which lies between the ftop-wire and and guide-wire, and turned two or three times round the other thread ; and then each thread is placed in the groove of the pulley at *b b*, taking care to place each in the groove of that pulley which lies on the fide of its own ftop-wire. The threads will then appear in the fituation reprefented in the figure twifted round one another in the two points *o* and *p*.

The great advantage of this method is the taking off the preffure of the threads, by making them pafs over two pullies inftead of two hooks ; and by the fwivel complying with all the motions of the guideftick, which keeps the angles of the croiffure conftantly the fame : without this, the bearing of the threads and the variation of the angles will make them fo liable to break, that, from many experiments which I have made, I might venture to fay,
that

that tho' the double croiffure has many advantages in compacting the filk thread, and fhaking off its moifture, yet without fuch contrivance it will fcarce ever be put in practice. I muft alfo take notice, that this contrivance is vaftly more fimple than that which Monf. Vaucançon ufes, tho' his has no one of thefe advantages. A defcription of his method may be feen in the treatife which I lately publifhed on the culture of filk, for the ufe of our American colonies, fold by Mr. Millar in the Strand ; but I had not at that time thought of thefe improvements.

N. B. In *Tab*. III. *fig*. 2. the fwivel-crofs is reprefented fomewhat large, and feparate from the reel, the better to diftinguifh its parts; but at B, *fig*. 1. it is fhewn in its true proportion and fituation when the reel is ready to work.

V. *Experiments on feveral Pieces of Marble ftained by Mr.* Robert Chambers. *In a Letter to the Rev.* Thomas Birch, *D. D. Secret. R. S. from Mr.* Emanuel Mendez da Cofta, *F. R. S.*

Rev. Sir,

I Take the liberty to addrefs to you fome notices on the art of ftaining or painting of marble, and the experiments I made on thofe pieces of painted marble produced before this Society, at their meetings on the 21ft December and 11th January laft.   The

The artiſt, whom I alſo introduced at the ſame time to the Society, is Mr. Robert Chambers, of Minching-Hampton in Glouceſterſhire ; and at my ·deſire he was preſent at the experiments I made on his ſaid painted marbles.

But before I relate the experiments, it may not be improper to give ſome little hiſtorical account of the art itſelf: it will at leaſt be amuſing to the Society.

Kircher, in his *Mundus Subterraneus*, lib. viii. ſect. 1. c. 9. p. 45 & 46. is the firſt author I know, who mentions it. There was, ſays he, an artiſt at Rome, who painted ſeveral pieces of marble, in an elegant manner, for Pope Urban VIII. He would not diſ-cover his art ; therefore Kircher ſtrove by many ex-periments to diſcover it : and he made colours, *viz.* tinctures of metals and minerals, which coloured the marble as finely as any the artiſt had done, and quite penetrated the ſtone ; inſomuch that a ſlab cut horizontally made as many pictures as pieces or ſec-tions. Kircher gives at large the proceſs he uſed for making the colours ; and obſerves, they ſhould always be of a mineral origin : which I incline alſo to believe would anſwer much the beſt.

The ſaid author *(Ibid.)* alſo gives another method to colour marble, by vitriol, bitumen, *&c.* forming a deſign of what you like upon paper, and laying the ſaid deſign between two pieces of poliſhed marble ; then cloſing all the interſtices with wax, you bury them for a month or two in a damp place. On taking them up, you will find, that the deſign you painted on the paper has penetrated the marbles, and formed exactly the ſame deſign on them. A modern author,

author, Wallerius, in his *Mineralogy*, vol. ii. gen. 58. p. 128. alfo recommends this method.

In the *Philofophical Tranfactions*, N°. 7. the firft method of Kircher is copied. The editor however. therein fays, that method has not fince been tried. He adds, that one Mr. Bird had for many years (he writes in 1666) found out a way to fink colours a confiderable depth into polifhed marble ; pieces whereof were fhewn to King Charles II. foon after his reftoration ; and, being broken in his prefence, it was found, that the colours had penetrated deep into the marbles ; and that many works of his coloured marbles were feen at Oxford and London. But Mr. Bird's way of doing it is not mentioned.

In the *Philofophical Tranfactions*, N°. 268. is a paper, intituled, " The Way of Colouring Marble." The anonymous author gives us an account of the colours, *&c.* he ufed. It is obfervable they are only vegetable colours. His red, he fays, he extracted again from the marble, without hurting the polifh, within fix and twenty hours, with oil of tartar *per deliquium*; and his brown was quite difcharged by aquafortis within one quarter of an hour, and the polifh of the marble quite deftroyed.

I fhall now proceed to give an account of the ex-periments I made. I could not well fuggeft any more, as the method of colouring the marble, the materials of the colours, *&c.* are kept fecret by the artift, Mr. Chambers.

A piece of marble, with the feveral colours ufed, on it, like a painter's pallet, being greatly faturated with aquafortis, at different times, for twenty hours,

tho'

tho' the polifh of the marble was quite effaced, yet there was not the leaft difcharge of any of the colours, nor were they any-wife dulled, &c.

N°. 6. A deep crimfon-red colour, being left twenty hours in a ftrong lye of common foft green foap, fuffered no change; and boiled in the fame lye half an hour, alfo fuffered no change. The marble finely powdered, and aqua fortis effufed over it, the marble particles were nigh deftroyed; but feveral red particles (no doubt the colour) remained. The marble, by common calcination, *i. e.* in a common coal fire, for half an hour, is intirely difcharged of its colour. We made the experiments on four other reds, and the refult was much the fame as abovefaid; fo that this is a ftandard for his reds.

N°. 5. A deep fea-green, being left twenty hours in a ftrong lye of common foft green foap, fuffered no change; but boiled in the fame lye it quite difcharged its green colour: however, it yet remained flightly tinctured yellowifh. By common calcination the colour was quite difcharged. Some other greens were tried, and anfwered much the fame.

N°. 10. 15. & 16. Brownifh or terreftrial yellowifh colours, near to a clay colour, boiled in a ftrong lye of common foft green foap, they fuffered no change. By common calcination the colours were difcharged, but retained a greyifh caft. Thefe colours, covered for forty-eight hours with a layer of the faid common foap, fuffered no fenfible change.

N°. 19. A bright yellow, boiled in a ftrong lye of common green foft foap, fuffered no change; and covered with a layer of the fame foap for forty-eight hours, the colour is dulled. By common cal-

F   cination

cination the colours are difcharged, but retain a greyifh caft. Several other different fhades of yellow anfwered much the fame by my experiments.

For blue, Mr. Chambers has not as yet ftained any marble of that colour.

By the above experiments we may conclude, that thefe colours are good, penetrate the marble freely without injuring it, remain uninjured by menftrua, &c.; and that only calcination difcharges them. Therefore it is probable, that Mr. Chambers's method of ftaining or colouring marbles is extremely good.

Tho' acid menftrua work greatly on marble, yet it is obfervable thefe colours are not difcharged by them, but only by calcnation; which, as it intirely and thoroughly deftroys the compages of the ftone, the fubftances of the colours muft undoubtedly at the fame time be exhaled by the force of the fire. We obferve a like procefs in the works of nature; *viz.* in the dendritæ; I mean, fuch as are on alcaline ftones: for tho' the ftones are utterly corroded by the acids, yet the dendritæ, however merely fuperficial, remain; but if calcined, the faid dendritæ are immediately exhaled, and intirely difappear.

This art will not only give pleafure to the eye by regular paintings (whereas the natural colourings of marble are very irregular), but it may be very ufeful to blazon arms, and for infcriptions; as fculpture alone can never exprefs colours, and chiffel'd infcriptions, &c. fuffer much by age: for probably a monument of marble, rightly coloured by this method, will be preferved ages from the injuries of the weather, tho'

tho' at the same time the stone itself will be somewhat hurt or corroded by the air.

I have the honour to be,

Reverend Sir,

Your very obliged and humble Servant,

Bearbinder-lane,   Emanuel Mendez da Costa.
7th February 1759.

VI. *Observations upon the* Sea Scolopendre, *or* Sea Millepes. *By* John Andrew Peyssonel, *M. D. F. R. S. Translated from the* French.

Read Feb. 15, 1759.
THIS creature, in its figure, is like the Land Scolopendre, or, as Pliny says, to the hairy caterpillar, commonly called the Milleped animal. It is of the same colour, has the same arrangement of circular rings : but whereas the Land Scolopendre is flat, this is square. I counted eighty rings, which form the body and head, when it was brought to me. This sea insect was very small, and almost imperceptible. I was surprised, after having kept him some time, to see a round body, of a blackish green colour, like the *glans virilis*, pass out of him, which had a considerable opening, like the canal of the urethra. This gland was surrounded by two bodies or bowels, which appeared in form of a prepuce turned back ; the one was yellowish, and the other whitish ; each but a
F 2   line

line thick, ſtronger and larger above, and terminating
below like a ligature, filled with a matter like that
contained in the inteſtines of fiſhes and inſects. This
may be what gave occaſion to Pliny, and other na-
turaliſts, to think, that theſe inſects, finding them-
ſelves taken, throw out their bowels in order to leſſen
their bulk.

It is very likely, that this gland, which it puſhes
out, and theſe kinds of bowels full of the ſame
matter, which fills them within, which in the
natural ſtate of the inſect do not at all appear, but
are in the inſide of the animal, without any ſigns of
their exiſtence, being puſhed outwards, increaſe the
ſize of its head to ten times its common bulk; and
are the inteſtines, which he throws out at times. Is
not the gland his ſtomach, and theſe prepuces his
guts, which he protrudes and retracts at pleaſure?ʳ
To what purpoſe this play or action of theſe parts?
I profeſs I do not know. It cannot be to diſengage
himſelf from the machine; and this muſt be very
ſmall, in order to be enabled to catch theſe inſects.
If it was of glaſs, and of a bell-form, the protruſion
of the ſtomach might be ſeen, in order to eject that
matter, whatſoever it be, in this play or action;
which may perhaps be hereafter diſcovered.

The body of the animal is ſquare; and the four
ſides are armed with ſuch prickles, as I never ſaw
before. Alſo every ring has four bundles of prickles.
The following is the manner of their being diſpoſed:
At the end of each ring, above the ſquare, on each
ſide, we ſee a gland, near which a bundle of prickles
ariſes towards the ſquares below. This bundle ſeems
to fill the whole ring, and to part as it were from the
center

center of the under fide ; where is a hollow or fepa-
ration, which paffes in a right line from the head to
the tail. The bundles of prickles appear round at
firft, and terminate in points ; but afterwards they
are feen to fpread out like a fan ; fo that their extre-
mities are more than four times the bignefs of their
bafes. They contain an infinite number of prickles,
which are extremely fine, loofe, and brilliant, like an
aigrette of glafs, but more free and loofe. The range
of prickles underneath fpreads alfo like fans, ferving
the infect as feet ; for it is upon thefe he ftands, and
moves upon them as the Scolopendre does on his
feet.

Having put thefe Sea Scolopendres upon my fin-
gers, they thruft a great number of their prickles
into the fkin, and caufed a fharp pain for fome hours :
it was like fire upon the part. It was in vain that I
rubbed and wafhed the part ; and tho' the prickles
were broken, yet the parts, that ftuck in the flefh,
produced their effect, and caufed the pain I felt for
fome hours. Afterwards it all went off without any
further ill confequence.

VII. *An*

VII. *An Account of a Storm of Thunder and Lightning* at Norwich, *on the* 13th *of* July 1758. *By Mr.* Samuel Cooper *.* *Communicated by Mr.* Joseph Warner, *Surgeon of* Guy's Hospital, *and F. R. S.*

Read Feb. 15, 1759.

ABOUT four o'clock on Thursday afternoon, July 13th 1758. a short but severe thunder-storm, with lightning, fell upon the top of an house standing alone, and belonging to a common garden, on the causeway near Sandling's-ferry, in the city of Norwich; struck off the tiles of the roof at the east end, to the space of a yard or two; burnt a very small hole in the middle of a lath, in piercing into the chamber, and then darted to the north-east; ript off the top of an old chair, without throwing it down; snapt the two heads of the bed-posts, rent the curtains, drove against the wall (the front of the house stands due north-east), forced out an upright of a window frame a yard long, three inches broad, and two thick; smote it in a right line into an opposite ditch, ten or twelve yards distant: then struck down on the wall of the chamber, paring off half a foot's breadth of its plaistered covering quite down to the floor; lifted up a board of the floor, and leaving an hole of half an inch diameter, pierced thro' by the side of the main

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* This account is confirmed in almost every circumstance by another communicated to the Royal Society in a letter from Mr. William Arderon, F. R. S. to Mr. Henry Baker, F. R. S.

beam

beam into the kitchen, towards the weft end of a pewter-fhelf; traverfed the whole fhelf to the eaft, and melted fuperficially to the breadth of a fhilling fix pewter difhes, two plates, and a pewter bafon, all ftanding touching one another: two of the difhes were thrown down, the reft not difplaced. Under this, a narrower fhelf of pewter plates untouched. In its defcent to the floor, knocked down, as fhe exprefles it, an ancient woman fitting in the paffage weftward of the fhelves; who being prefently taken up, her fhoulders and back were found to be fcorched all over, with the hind part of her left leg: the fkin almoft univerfally red and inflamed, rimpled in two or three places, but not broken: her fhift burnt brown, ftocking finged, with its colour of the infide difcharged, and the outfide unchanged: right foot very painful and bruifed, with that fhoe ftruck off, and its upper-leather torn: her gown and other cloaths without any damage. It paffed through the fame paffage without injuring another old woman fitting knee to knee with her companion; but keeping its direction to the north-eaft, turned on a right angle upon the outer door, fplit it, and paffed through into the open air. On a right line with this paffage to the weft, and under the fame roof, is the wafh-houfe, where ftood the mafter and his man. They faw the woman tumble down, and heard fuch a violent explofion, that made them both think the whole houfe muft come down: and the man fays, with fuch a blaze, as if all was on fire; but that was but for a moment. To the eaft of the pewter fhelves, and under that part of the roof, where it entered, it rufhed into the kitchen clofet, by tearing off a wooden button

7

ton, that was nailed on, and there took some pieces from a Delft dish without throwing it down, broke a quart mug, and from a four-ounce phial half full of oil cut off its empty half part without spilling a drop of the oil. The activity of the lightning was with abated violence to all other points of the compass; but not without some considerable degree of force; for it scraped the plaister off the wall in many different and distant places, both in the chamber and kitchen: and to the south-west of the chamber, where was the window, broke many panes of glass, and tore the lead outwardly, without melting it; and broke two panes of the kitchen window, with its lead, situated under the chamber window. Both kitchen and chamber smelt as strong of sulphur some hours after, as if fumigated with brimstone matches.

<div align="right">Sam. Cooper.</div>

VIII. *Experiments concerning the* Encaustic Painting *of the Ancients. In a Letter to the Right Honourable* George *Earl of* Macclesfield, *President of the Royal Society, from Mr.* Josiah Colebrooke, *F. R. S.*

<div align="center">My Lord,</div>

Read Mar. 1. 1759. THE result of experiments (whatever the success attending them may be), in philosophical or mechanical inquiries, is not below the attention of the Royal Society.

<div align="right">The</div>

The art of painting with burnt wax, (as it is called) hath long been loft to the world; the ufe of it to painters, in the infancy of the art of painting, was of the utmoft confequence, drying oil being unknown, they had nothing to preferve their colours entire from the injury of damps, and the heat of the fun; a varnifh of fome fort was therefore neceffary; but they being unacquainted with diftilled fpirits, could not, as we now do, diffolve gums to make a tranfparent coat for their pictures; this invention therefore of burnt wax fupplied that defect to them, and with this manner of painting, the chambers and other rooms in their houfes were furnifhed; this Pliny calls *encauftum*, and we encauftick painting.

The following experiments which I have the honour to lay before your Lordfhip and the Society, were occafioned by the extract of a letter from the Abbé Mazeas, tranflated by Dr. Parfons, and publifhed in the fecond part of the 49th volume of the *Philofophical Tranfactions*, N° 100, concerning the antient method of painting with burnt wax, revived by Count Caylus.

The Count's method was,

Firft, To rub the cloth or board defigned for the picture fimply over with bees-wax.

Secondly, To lay on the colours mixed with common water; but as the colours will not adhere to the wax, the whole picture was firft rubbed over with * Spanifh chalk, and then the colours are ufed.

Vol. LI.   G   *Thirdly,*

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* Spanifh chalk is called by Dr. Parfons, in a note, Spanifh white; this is a better kind of whitening than the common, and was the

only

Thirdly, When the picture is dry, it is put near the fire, whereby the wax melts, and abforbs all the colours.

Experiment I.

A piece of oak board was rubbed over with bees-wax, firft againft the grain of the wood, and then with the grain, to fill up all the pores that remained after it had been planed, and afterwar s was rubbed over with as much dry Spanifh white, as could be made to ftick on it; this, on being painted (the colours mixed with water only), fo clogged the pencil, and mixed fo unequally with the ground, that it was im-poffible to make even an outline, but what was fo much thicker in one part than another, that it would not bear fo much as the name of painting; neither had it any appearance of a picture: However, to purfue the experiment, this was put at a diftance from the fire, on the hearth, and the wax melted by flow degrees; but the Spanifh white (tho' laid as fmooth as fo foft a body would admit, before the colour was laid on) yet on melting the wax into it, was not fufficient to hide the grain of the wood, nor fhew the colours by a proper whitenefs of the ground, the wax in rubbing on the board, was unavoidably

only white that had the name of Spanifh annexed to it, that I could procure, tho' I enquired for it at moft if not all the colour fhops in town.

My friend Mr. Dacofta fhewed me a piece of Spanifh chalk in his collection, which feemed more like a *cimolia* (tobacco pipe clay), and was the reafon of my ufing that in one of the experiments.

thicker

thicker in fome parts than other, and the Spanifh
white the fame : on this I fufpected there muft be
fome miftake in the Spanifh white and made the en-
quiry mentioned in the note * pag. 41.

To obviate the inequality of the ground in the
firft experiment;

EXPERIMENT II.

A piece of old wainfcoat (oak board) ¼ of an inch
thick, which having been part of an old drawer,
was not likely to fhrink on being brought near the
fire ; this was fmoothed with a fifh-fkin, made quite
warm before the fire, and then with a brufh dipped
in white wax, melted in an earthen pipkin fmeared
all over, and applied to the fire again, that the wax
might be equally thick on all parts of the board, a
ground was laid (on the waxed board) with levigated
chalk mixed with gum water (*viz.* Gum Arabick
diffolved in water): When it was dry, I painted it with
a kind of landfkip, and purfuing the method laid
down by Count Caylus, brought it gradually to the
fire; I fixed the picture on a fire fcreen which would
preferve the heat, and communicate it to the back
part of the board, this was placed firft at the dif-
tance of three feet from the fire, and brought for-
ward by flow degrees till it came within one foot of
the fire, which made the wax fwell and bloat up the
picture; but as the chalk did not abforb the wax, the
picture fell from the board and left it quite bare.

EXPE-

Experiment III.

I mixed three parts white wax, and one part white refin, hoping the tenacity of the refin might preferve the picture. This was laid on a board heated, with a brufh, as in the former; and the ground was chalk, prepared as before. This was placed horizontally on an ironing box, charged with an hot heater, fhifting it from time to time, that the wax and refin might penetrate the chalk; and hoping from this pofition, that the ground, bloated by melting the wax, would fubfide into its proper place: but this, like the other, came from the board, and would not at all adhere.

Experiment IV.

Prepared chalk four drams, white wax, white refin, of each a dram, burnt alabafter half a dram; were all powdered together and fifted, mixed with fpirit of moloffes inftead of water, and put for a ground on a board fmeared with wax and refin, as in *Exp.* 3. This was alfo placed horizontally on a box-iron, as the former: the picture bliftered, and was cracked all over; and tho' removed from the box-iron to an oven moderately heated (in the fame horizontal pofition), it would not fubfide, nor become fmooth. When it was cold, I took an iron fpatula made warm, and moved it gently over the furface of the picture, as if I were to fpread a plaifter. (This thought occurred, from the board being prepared with wax and refin, and the ground having the fame materials in its compofition, the force of the

fpatula

fpatula might make them unite.) This fucceeded fo well, as to reduce the furface to a tolerable degree of fmoothnefs: but as the ground was broke off in many places, I repaired it with flake white, mixed up with the yolk of an egg and milk, and repainted it with moloffes fpirit (inftead of water); and then put it into an oven with a moderate degree of heat. In this I found the colours fixed, but darker than when it was firft painted; and it would bear being wafhed with water, not rubbed with a wet cloth.

Experiment V.

A board (that had been ufed in a former experiment) was fmeared with wax and refin, of each equal parts; was wetted with moloffes fpirit, to make whitening (or Spanifh white) mixed with gum-water adhere. This, when dry, was fcraped with a knife, to make it equally thick in all places. It was put into a warm oven, to make the varnifh incorporate partly with the whitening before it was painted; and it had only a fmall degree of heat: water only was ufed to mix the colours. This was again put into an oven with a greater degree of heat; but it flaked off from the board: whether it might be owing to the board's having had a fecond coat of varnifh (the firft having been fcraped and melted off), and that the unctuous parts of the wax had fo entered its pores, that it would not retain a fecond varnifh, I cannot tell.

Experiment VI.

Having mifcarried in thefe trials, I took a new board, planed fmooth, but not polifhed, either with
a fifh-

a fifh-fkin or ruſhes: I warmed it, and ſmeared it
with wax only; then took *cimolia* (tobacco-pipe
clay) divefted of its ſand, by being diſſolved in wa-
ter and poured off, leaving the coarſe heavy parts be-
hind. After this was dried and powdered, I mixed
it with a ſmall quantity of the yolk of an egg and
cow's milk, and made a ground with this on the
waxed board: this I was induced to try, by know-
ing that the yolk of an egg will diſſolve almoſt all
unctuous ſubſtances, and make them incorporate
with water; and I apprehended, that a ground, thus
prepared, would adhere ſo much the more firmly to
the board than the former had done, as to prevent
its flaking off. The milk, I thought, might anſwer
two purpoſes; firſt, uniting the ground with the
wax; and ſecondly, by anſwering the end of ſize,
or gum-water, and prevent the colours ſinking too
deep into the ground, or running one into another.
When the ground was near dry, I ſmoothed it with
a pallat-knife, and waſhed with milk and egg where
I had occaſion to make it ſmooth and even: when
dry I painted it, mixing the colours with common
water; this, on being placed horizontally in an oven,
only warm enough to melt the wax, flaked from
the board; but held ſo much better together than
any of the former, that I paſted part of it on
paper.

Experiment VII.

* Flake white mixed with egg and milk, crumbled all to pieces in the oven, put on the waxed board, as in the laſt experiment.

The bad ſucceſs which had attended all the former experiments, led me to conſider of what uſe the wax was in this kind of painting: and it occurred to me, that it was only as a varniſh to preſerve the colours from fading.

In order to try this,

Experiment VIII.

I took what the bricklayers call fine ſtuff, or putty †; to this I added a ſmall quantity of burnt alabaſter, to make it dry: this it ſoon did in the open air; but before I put on any colours, I dried it gently by the fire, left the colours ſhould run. When it was painted, I warmed it gradually by the fire (to prevent the ground from cracking), till it was very hot. I then took white wax three parts, white reſin one part, melted them in an earthen pipkin, and with a bruſh ſpread them all over the painted board, and kept it cloſe to the fire in a perpendicular ſituation, that what wax and reſin the plaiſter would not abſorb might drop off. When it was cold, I found the colours were not altered, either from the

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* Flake white is the pureſt ſort of white lead.
† Putty is lime ſlacked, and, while warm, diſſolved in water, and ſtrained through a ſieve.

heat

heat of the fire, or paffing the brufh over them. I then rubbed it with a foft linen cloth, and thereby procured a kind of glofs, which I afterwards increafed by rubbing it with an hard brufh; which was fo far from fcratching or leaving any marks on the picture, that it became more fmooth and polifhed by it.

After I had made all the foregoing experiments, in converfation with my honoured and learned friend Dr. Kidby, a fellow of this Society, I faid I had been trying to find out what the encauftic painting of the ancients was. Upon which, he told me, that there was a paffage in Vitruvius *de architectura* relative to that kind of painting; and was fo good as to tranfcribe it for me from the 7th book, chap. 9. *De minii temperatura.* Vitruvius's words are; *At fi quis fubtilior fuerit, & voluerit expolitionem miniaceam fuum colorem retinere, cum paries expolitus & aridus fuerit, tunc ceram punicam liquefactam igni, paulo oleo temperatam, feta inducat, deinde poftea carbonibus in ferreo vafe compofitis, eam ceram apprime cum pariete, calefaciendo fudare cogat, fiatque ut peræquetur, deinde cum candela linteifque puris fubigat, uti figna marmorea nuda curantur. Hæc autem* καυσις *græce dicitur. Ita obftans ceræ punicæ lorica non patitur, nec lunæ fplendorem, nec folis radios lambendo eripere ex his politionibus colorem.*

Which I thus tranflate: " But if any one is more
" wary, and would have the polifhing [painting]
" with vermilion hold its colour, when the wall is
" painted and dry, let him take Carthaginian [Barbary] wax, melted with a little oil, and rub it on
" the wall with an hair pencil; and afterwards let
<div align="right">" him</div>

" him put live coals into an iron veſſel (chafing-diſh),
" and hold it cloſe to the wax, when the wall, by
" being heated, begins to ſweat ; then let it be made
" ſmooth : afterwards let him rub it with a * candle
" and † clean linen rags, in the ſame manner as they
" do the naked marble ſtatues. This the Greeks call
" καυσις. The coat of Carthaginian wax (thus put
" on) is ſo ſtrong, that it neither ſuffers the moon
" by night, nor the ſun-beams by day, to deſtroy
" the colour."

Being ſatisfied, from this paſſage in Vitruvius,
that the manner of uſing wax in *Exp*. 8. was
right, I was now to find if the wax-varniſh, thus
burnt into the picture, would bear waſhing : but
here I was a little diſappointed ; for rubbing one
corner with a wet linen cloth, ſome of the colour
came off ; but waſhing with a ſoft hair pencil dipped
in water, and letting it dry without wiping, the co-
lours ſtood very well.

A board painted, as in *Exp*. 8, was hung in the
moſt ſmoaky part of a chimney for a day, and ex-

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* This account of the method of poliſhing [painting] walls
coloured with vermilion, gave me great ſatisfaction, as it proved
the method I had taken in experiment 8. (which I had tried
before I ſaw or knew of this paſſage in Vitruvius) was right. The
uſe of the candle, as I apprehend, was to melt the wax on the
walls, where by accident the bruſh had put on too much, or afford
wax where the bruſh had not put on enough, or had left any part
bare.

† The rubbing the wall with a linen cloth, while warm, will
do very well, where there is only one colour to be preſerved ; but
where there are many, as in a landſchape, it will be apt to take off
ſome, or render the colouring rather faint ; which I found by
wiping the wax off from a painting while it was hot.

poſed

pofed to the open air in a very foggy night. In the morning the board was feemingly wet through, and the water ran off the picture. This was fuffered to dry without wiping; and the picture had not fuffered at all from the fmoke or the dew, either in the ground or the colours: but when dry, by rubbing it, firft with a foft cloth, and afterwards with a brufh, it recovered its former glofs.

Sufpecting that fome tallow might have been mixed with the white wax I had ufed, which might caufe the colours to come off on being rubbed with a wet cloth, I took yellow wax which had been melted from the honeycomb in a private family, and confequently not at all adulterated; to three parts of this I added one part refin, and melted them together.

EXPERIMENT IX.

Spanifh white, mixed with fifh glew, was put for a ground on a board, and painted with water colours only. The board was made warm; and then the wax and refin were put on with a brufh, and kept clofe to the fire till the picture had imbibed all the varnifh, and looked dry. When it was cold, I rubbed it firft with a linen cloth, and then polifhed it with an hard brufh.

In thefe experiments I found great difficulties with regard to colours; many water colours being made from the juices of plants, have fome degree of an acid in them; and thefe, when painted on an alkaline ground, as chalk, whitening, *cimalia*, and plaifter, are, totally changed their colours, and, from green became

became brown; which contributed much to make the experiments tedious. I would therefore advife the ufe of mineral or metallic colours for this fort of painting, as moft likely to preferve their colour: for although I neutralized Spanifh white, by fermenting it with vinegar, and afterwards wafhed it very well with water, it did not fucceed to my wifh.

Thefe experiments, and this paffage from Vitruvius, will in fome meafure explain the obfcurity of part of that paffage in Pliny which Dr. Parfons, in his learned comment on the encauftic painting with wax, feems to defpair of.

Ceris pingere was one fpecies of encauftic painting. Εγκαυστον, *inuftum*, may be tranflated, forced in by the means of fire, burnt in: for whatever is forced in by the help of fire can be rendered into Latin by no other fignificant word, that I know of, but *inuftum*. If this is allowed me, and I think I have the authority of Vitruvius (a writer in the Auguftan age) for it, who feems to have wrote from his own knowlege, and not like Pliny, who copied from others much more than he knew himfelf, the difficulty with regard to this kind of painting is folved, and the encauftic with burnt wax recovered to the public.

What he means by the next kind he mentions, *in ebore ceftro id eft viriculo*, I will not attempt to explain at prefent.

The fhip painting is more eafily accounted for: the practice being, in part, continued to this time; and is what is corruptly called breaming, for brenning or burning. This is done by reeds fet on fire, and held under the fide of a fhip till it is quite hot;

then

then refin, tallow, tar, and brimftone, melted toge-
ther, and put on with an hair brufh while the planks
remain hot, make fuch a kind of paint as Pliny de-
fcribes ; which, he fays, *nec fole, nec fale, ventifque* •
corrumpitur, as they were ignorant of the ufe of oil
painting, they mixed that colour with the wax, *&c.*
which they intended for each particular part of the
fhip, and put it on in the manner above defcribed.

In the pictures painted for thefe experiments, and
now laid before your Lordfhip and the Society, I
hope neither the defign of the landfchape, nor the
execution of it, will be fo much taken into confi-
deration as the varnifh (which was the thing wanted
in this inquiry): and I think that will evince, that
the encauftic painting with burnt wax is fully reftored
by thefe experiments ; and though not a new inven-
tion, yet having been loft for fo many ages, and now
applied further, and to other purpofes than it was by
Vitruvius (who confined it to vermilion only), may
almoft amount to a new difcovery, the ufe of it may
be a means of preferving many curious drawings to
* pofterity : for this kind of painting may be on
paper, cloth, or any other fubftance that will admit
a ground to be laid on it. The procefs is very fimple,
and is not attended with the difagreeable fmell un-
avoidable in oil painting, nor with fome inconveniences
infeparable from that art ; and as there is no fubftance
we know, more durable than wax, it hath the greateft
probability of being lafting.

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* A bird painted by Mr. Edwards on paper, and the colours fixed
by burned wax, was fhewn to the Society, April 5th.

I afk

I aſk pardon of your Lordſhip and the Society for taking up ſo much time as this paper hath required: but if it meets with your Lordſhip's and the Society's approbation, I may, in ſome future paper (when the neceſſary avocations of my profeſſion will allow me leiſure), lay before you ſome experiments, relating to colours which are not likely to change by being painted on any kind of ground.

As your Lordſhip's recommendation contributed much to make me a member of this learned body, I muſt beg your patronage of this communication; and am, with the greateſt reſpect,

<div align="center">Your Lordſhip's and the Society's</div>

<div align="center">Moſt obedient humble Servant,</div>

Budge-row,   Joſiah Colebrooke.
February 27, 1759.

IX *A Letter concerning the ſucceſs of the preceding Experiments. In a Letter to the Right Honourable Lord* Charles Cavendiſh, *V. P. R. S. from Mr.* Joſiah Colebrooke, *F. R. S.*

<div align="center">My Lord,</div>

Read April 5
1759.
IN a paper (I lately had the honour to lay before the Royal Society, on the encauſtic painting of the ancients) I mentioned an uſe which might be made of it to preſerve drawings. I have now the pleaſure of laying before your Lordſhip

ſhip and the Society a ſpecimen of the encauſtic upon paper, being a bird drawn by Mr. George Edwards, a fellow of this Society, on paper prepared with a ground of whitening and fiſh-glew, painted with water colours, and then the wax, &c. burned in. This will roll up as eaſily as common paper, without cracking the varniſh. There are alſo two landſchapes, painted by a young lady, after the ſame manner, on wood. Theſe will fully evince all I advanced in that paper.

I am, my Lord, with the greateſt reſpect,

Your Lordſhip's moſt humble Servant,

Budge-row,
April 5. 1759.

Joſiah Colebrooke.

X. *An Account of a particular Species of* Cocoon, *or* Silk-pod, *from* America. *By the Reverend* Samuel Pullein, *M. A.*

Read Mar. 8, 1759. HAVING lately ſeen the aurelia of a particular ſpecies of caterpillar, I judged, from its texture and conſiſtence, that there might be procured from it a ſilk not inferior to that of the common ſilk-worm in its quality, and in its quantity much ſuperior. I have made ſome experiments on this new ſpecies of ſilk-pod, which ſtrengthen this opinion.

This pod is about three inches and a quarter in length, and above one inch in diameter ; its outward form not
ſo

fo regular an oval as that of the common filk-worm; its confiftence fomewhat like that of a dried bladder, when not fully blown; its colour of a red-difh brown; its whole weight 21 grains.

Upon cutting open this outer integument, there appeared in the infide a pod completely oval, as that of the filk-worm. It was covered with fome flofs-filk, by which it was connected to the outer coat, being of the fame colour. Its length was two inches; its diameter nearly one inch; and its weight nine grains.

The pod could not be eafily unwinded, becaufe it was perforated by the moth: but, upon putting it in hot water, I reeled off fo much as fufficed to form a judgment of the ftrength and ftaple of its filk.

The fingle thread winded off the pod in the fame manner as that of the common filk-worm; feeming in all refpects as fine, and as tough. I doubled this thread fo often as to contain twenty in thicknefs; and the compound thread was as fmooth, as ela-ftic, and as gloffy, as that of the common filk-worm. I tried what weight it would bear; and it bore fifteen ounces and a half, and broke with fomewhat lefs than fixteen, upon feveral trials. I then tried a thread of the common filk-worm, which was alfo compofed of twenty (in thicknefs it rather exceeded the other); and it broke always with fifteen ounces.

I boiled a part of the *cocoon* in water, for the fpace of four hours, that I might know whether it was compofed of a gum in any fort mucilaginous;

and

and I found that it was as indiffoluble as that of the common filk-worm.

The common filk-pod, with all its flofs, weighs ufually but three grains : and here is a pod which, weighs feven times as much. If the outer coat, which weighed twelve grains, were all to be ufed only as flofs-filk, there remain nine grains, capable of being reeled ; which is above three times as much as can be reeled from the common *cocoon*. But I am of opinion, that when the pod is frefh, and not hardened by age, the whole outer coat may be reeled off: for the pod on which I made thefe trials was feven or eight years old.

Upon enquiry, I have found that the moth of this pod is called the Ifinglafs by Marian. It is a very large moth, being five inches from the tip of each wing extended. It differs from the filk-moth, in that it has a probofcis ; which intimates that it feeds in its papilio ftate, whereas the filk-moth never eats.

The caterpillar which produces this pod is a native of America. It was found in Penfylvania : the pod was fixed to the fmall branch of a tree, which feemed to be either of the crab or hawthorn fpecies.

The leaf of the tree had alfo helped to fupport the pod ; for the mark of its ribs was apparent on the furface of the pod.

I do not conceive that it will be at all difficult to find out the caterpillar, or the tree it feeds on ; or to reel fuch a quantity of the filk as fhall, when woven into ribband, more fully demonftrate whether it be of that value which I judge it. For by

3   comparing

comparing it with the *cocoon* of the wild Chinefe filk-worm, from which an excellent fpecies of filk is made, I have no doubt of its being the fame fpecies; and would be glad if, by this memorial, I could induce the people of America to make trial of it.

Samuel Pullein.

XI. *A Thermometrical Account of the Weather, for One Year, beginning* September 1753. *Kept in* Maryland, *by Mr.* Richard Brooke, *Physician and Surgeon in that Province. Communicated by Mr.* Henry Baker, *F. R. S.*

Read Mar. 13,
1755.

SEPTEMBER, 1753.

A.M		Wind and Weather.	P.M.		Wind and Weather.
☿			☿		
	79	Clear. N.W. moderate	79		Clear. S.E.
	81	Flying clouds. S.E.	87		Dº Dº
	72	Dº N.W.	80		Dº N.W.
	70	Dº S.E.	76		Dº S.E.
	70	Dº S.W.	77		Rained. S.W.
	69	Clear and calm	72		Clear and calm
	83	Dº Dº	87		Dº S.
	74	Cloudy. N.E.	82		Cloudy. N.E.
9		Dº Calm	79		Wind high. S. Rained
10		Dº Dº	75		Hard rain. Evening clear
11		Dº Dº	65		Flying clouds. N.E.
12	64	Dº Dº			*Intermittent fever and flux*
		Dº S.E.	68		Dº Dº
23	70	Fl. clouds. N.W. brisk gal			Flying clouds. N.W. Dº
24	60	Rained. N.E.	63		Rained. N.E. Dº
25	62	Fair and calm			Fair
26	61	Dº Dº	71		Wind. S.E. Dº
27	67	Flying clouds. Calm	73		Fl. clouds and calm. Dº
28	66	Dº N.E.	67		Calm. Rained. Dº
29	63	Dº Dº	72		Flying clouds. N.E. Dº
30	58¼	Clear and calm	73		Clear and calm. Dº

OCTOBER.

OCTOBER.

	A.M ☿	Wind and Weather.	P.M. ☿	Wind and Weather.	
1	72	Foggy and calm	73	Misty	
2	64	Cloudy. D°	66	Rained. S.	
3	66	Flying clouds. S.	74	Flying clouds. S.	
4	70	Thunder and rain. S.	71	Thunder and lightening	
5	64	Flying clouds. N. W.	74	Flying clouds. N. W.	
7	56	Cloudy. D°	64	Cloudy. D°	
8	54	Rained at times. D°	62	D° Calm	
9	52	Cloudy. D°	56	D° D°	
10	52	Rained. D°	64	Clear and D°	
11	56	Flying clouds. S.			Worms in children
14	62	Brisk gale. N. W.	68	Brisk gale. N.W.	more common than usual,
17	61	Cloudy. Calm			from in-
18	64	Flying clouds. S. E.	65	Clear. S.	fancy to
19	66	D° S.			the age of
20	62	Cloudy and calm	71	Sunshine. S. E.	fourteen
21	66	Rained. E.			or fifteen
23	57	Flying clouds. N. E.			
27	58	Brisk gale. N. W.	59	Brisk gale. N. W.	
28	47	Flying clouds. D°	49	Flying clouds D° D°	
29	44	Frost. D°	52	Cloudy. S. E.	
30	42	Rained. Snowed. N.	36	Snowed. N. W.	
31	47	Clear. High. N. W.	51	Clear. High. D° D°	

NOVEM-

NOVEMBER.

	A.M	Wind and Weather.	P.M	Wind and Weather.
	♀		☿	
1		Clear and calm. N. W.	61	Clear. E.
2	58	Flying clouds. S. E.	63	Rained. Brisk gale. S. E.
3		Clear. High. N.W.	39	Clear. D° N W.
4	41	D° D° D°		D° D°
5	47	D° Calm		D° Calm. *Worm fevers*
7	50	D° D°	61	D° S. E.
8	57	Cloudy. E.	61	Cloudy. E.
9	58	Rained. F.	62	Rained. F.
10	60	D° N. W.	62	Clear. N. W.
11	55	Fair. D°	60	D° D°
13	47	Cloudy and calm	52	Rained. E.
14	43	Rain'd. High at times. S.W.	38	D° and snowed. S W.
15	36	Clear. D°	42	Clear. D°
16	40	D° D°	41	D° D°
17	39	Cloudy. D°	41	Cloudy. Calm
18	40	Clear and calm	42	Clear. D°
19	44	D° D°	50	D° D°
20	45	Cloudy. E.	51	Rained. E.
21	50	Fair. Calm.	53	Fair. Calm.
22	44	Flying clouds. High. N.W.	46	D° High. N.W.
23	42	Snowed. D°	45	D° D°
24	43	Thin clouds. S. E.	46	Thin clouds. E.
25	46	Rained. D°	48	Rained. E.
26	38	Clear. N.W.	44	Clear. N.W.
27	44	D° Calm	45	Cloudy. D°
28	53	D° S.	58	Clear. S.
29	48	D° N. W.	50	D° N.W.
30	52	D° S.W.	56	Fair and smoky. S.W.

DECEM-

DECEMBER.

	A.M ☿	Wind and Weather.		P.A.	Wind and Weather.	
1	55	Smoky and calm		5	Fair and calm.	
2	54	Rained.	E.	7	Rained.	E.
3	67	Cloudy and calm		9	Flying clouds	
4	60	Foggy.	S.	0	D°	S.
5	54	Cloudy. Rain.	N.	0	Cloudy.	N.E.
6	40	Rained.	N.E.	2	Rained	
7	48	D° Calm		9	D°	
8	49	Cloudy. D°		7	Cloudy	
9	39	Fair.	N.W.	8	Fair.	N W.
0	52	D°	D°	5	D°	D°
11	45	Rained.	N.E.	3	Rained.	N.E.
12	42	D°		0	D°	
13	47	Fair.	S.W	5	Fair.	N.W.
15	40	Rained : then Sunshine and fl. clouds. High. N.W.	D°	39	D° High.	D°
16	33	Clear. D°	D°	36	Fair.	D°
17	32	Snowed		31	D°	
18	32	Clear.	D°	8	D°	
19	33	D°	D°	1	D°	
20	30	Cloudy. Calm		9	D°	N.W.
21	35	Clear.	N.W.	2	D°	D°
22	35	Snowed.	D°		Flying clouds.	D°
23	35	Clear.	D°	41	Fair.	D°
24	36	Cloudy and calm		43	Rained. High.	E.
25	36	Flying clouds. High. S.W		40	Fl. clouds. High.	S.W.
26	11	Clear.	N.W.	17	Fair.	N.W.
27	15	Cloudy. Calm		25	Cloudy	
28	26	Snowed. D°		32	Hail	
29	33	Cloudy.	N.W.	32	Clear. High.	N.W.
30	11	Clear.	D°	29	D°	D°
31	29	D°	D°	37	D°	D°

JANU-

JANUARY, 1754.

	A M ☿	Wind and Weather.		P.M. ☿	Wind and Weather.	
1	21	Clear and calm		27	Clear.	S.
2	29	Cloudy. D°		37	Hail, rain, and snow.	S.
3	38	D°		41	Flying clouds	
4	36	Thin clouds. D°		41	D°	
5	39	Rained. Calm.		46	Rained.	E.
6	42	D° Snowed. D°		38	Clear. Brisk.	N.W.
7	2-	Clear. D°		48	D° Calm	
8	3=	D°	E.	45	D°	W.
9	51	Cloudy. Brisk.	S.	61	Flying clouds.	S.W.
10	36	Clear and calm		51	Clear.	W.
11	35	Thin clouds.	E.	58	Thin clouds.	S.W.
12	57	Rained.	S.W.	64	Rained.	D°
13	54	Foggy. Calm.		63	Clear.	N.W.
14	53	Thin clouds.	W.	63	Flying clouds	D°
15	49	Cloudy.	E.	50	Cloudy.	S.E.
16	61	Rained. High.	S.	55	Clear. High.	N.W.
17	39	Clear. Moderate.	N.W.	46	D° Calm	
18	37	D°	N.E.	48	D°	S.
19	37	D°	S.W.	50	D°	S.W.
20	49	D° Calm		62	D°	S.
21	57	Cloudy.	S.	42	D° High.	N.W.
22	15	Clear. Moderate.	N.W.	24	D° D°	D°
23	15	D°	D°	34	Cloudy.	S.
24	39	Snowed. Calm.		42	D°	N.
25	36	Cloudy.	N.W.	48	D°	N.W.
26	33	Clear.	S.	52	Clear.	S.
27	39	Thin clouds.	D°	61	D° Calm	
28	58	D°	D°	60	Rained	
29	44	Cloudy.	E.	46	D°	E.
30	42	Foggy.	N.W.	52	Clear. High.	N.W.
31	44	Thin clouds		46	Rained.	N.E.

FEBRU-

FEBRUARY.

	A.M ☿	Wind and Weather.	P.M ☿	Wind and Weather.
1	46	Drizzle.　Calm.	53	Cloudy.　S. E.
2	48	Foggy.　D°	54	Rained.　E.
3	45	D°	48	D°
4	44	Cloudy.　S. E.	46	Cloudy.　N.W.
5	33	Clear.　N.W.	45	Clear.　D°
6	33	Cloudy and calm	47	Cloudy and calm
7	42	Clear.　N.W.	47	Clear.　N.E.
8	39	Rained.　E.	49	Flying clouds.　S.W.
9	61	D°　High.　N.W.	52	Stormy weather
10	48	at 7 o'clock, 36° at 9 o'clock. Clear.　High.　N.W.		Clear.　High.　N.W.
11	32	Snowed, and calm	37	Snowed.　E.
12	10	Clear.　High.　N.W.	23	at 5 o'clock, 14° at 9 o'clock Clear.　N.W.
13	11	D°　Moderate.　D°	23	Clear.　D°
14	26	Cloudy ; various winds	39	D°　S.
15	36	Thin clouds.　S.W.	51	Smoky.　S.W.
16	49	Cloudy.　Brisk gale.　D°	36	Very high.　N.W.
17	24	Clear and calm	39	Clear.　S. E.
18	36	Hailed and rained	55	Cloudy.　S.
19	60	Cloudy.　High.　S.	45	Rained.　High.　N.W.
20	33	Clear.　Moderate.　D°	46	Clear.　S.
22	49	Cloudy.　S.	57	Cloudy.　E.
23	57	D°　D°	57	Flying clouds.　S.
24	47	D°　S. E.	59	Rained. Brisk gale. N.W.
25	47	Snowed.　N.	33	Snowed.　Westwardly
26	33	Drizzle	35	Drizzle
27	46	Cloudy.　E	50	Rained.　High.　N.W.
28		D°　Moderate.　N.W.	37	Clear.　D°

MARCH.

M A R C H.

	A.M ☿	Wind and Weather.		P.M. ☿	Wind and Weather.	
1	27	Clear.	N.	37	Snowed.	S. E.
2	37	Snowed		45	Rained	
3	37	Clear.	S.W.	62	Cloudy.	S.
4	52	D°	D°	71	Smoky.	S.W.
5	62	Smoky. Brisk.	D°	63	Thunder. Rain.	S.
6	49	Rained. Calm.		44	Rain. Calm	
7	42	Cloudy		45	D°	N.E.
8	39	Hail, rain, and snow.	N.E.	38	D° Hail and snow	D°
9	35	D° D°	D°	34	D° D°	Eastwardly
10	35	Drizzle	N.W.	34	Snowed.	N.W.
11	34	Clear. Brisk.	D°	48	Clouds.	Westwardly
12	41	Clouds.	S.E.		Smoky	
13		Smoky.	S.	71	D°	S.
14	63	D°	S.W.	69	D°	N.E.
15	53	D°	N.E.	55	D°	S. E.
16	57	D°	S.	66	D° Rained.	N.E.
17	47	Clear.	N. E.	49	Clear.	S.W.
18	43	Clouds.	S.	60	D°	S.
19	63	Smoky.	N.W.	63	D° Brisk gale.	N.W.
20	46	Clear.	D°	44	D°	S.E.
21	46	Rain. Thunder.	E.	43	Cloudy.	N.W.
22	38	Clear.	N.W.	44	D° Thunder and lightn.	
23		Rained.	E.	56	Smoky.	S.W.
24	53	D° Thunder		54	Clouds.	N.W.
25	48	Clear.	N.W.	53	Clear.	D°
26	42	D°	D°	45	D°	N.
27	33	D°	N.	51	D°	D°
28	47	Rained.	E.	47	Thund. lightn. rain.	N.E.
29	46	Cloudy and calm		48	Cloudy and calm	
30	45	Clear. D°		59	Clear.	E.
31	44	Thin clouds.	N.	55	Cloudy.	N.

A P R I L.

A P R I L.

	A.M ☿	Wind and Weather.		P.M. ☿	Wind and Weather.	
1	44	Clear. Various winds.		59	Clear.	E.
2	45	D°	E.	65	D°	D°
3	47	D°		63	D°	D°
4	51	Smoky.	S.	56	Cloudy.	D°
5	55	Rained.	E.	53	Rained.	N. E.
6	52	D°	N. E.	53	Cloudy.	D°
7	50	Clear.	D°	56	Rained.	D°
8	52	Cloudy.	D°	54	Clear.	D°
9	53	Drizzle.	D°	56	Clouds.	D°
10	57	Cloudy.	S. E.	62	Rained.	S.
11	57	Flying clouds.	N.W.	51	Fl. clds. Brifk. N.W. Froft	
12	42	Clear. Brifk.	D°	47	Clear. D° D° D°	
13	47	D° D°	D°	50	D° moderate. D° D°	
14	48	D° Calm		59	Cloudy.	E.
15	54	Clouds.	E.	56	Rained.	D°
16	54	Rained.	D°	52	Clouds.	N. E.
17	44	Clear.	N.W.	45	Clear. Brifk. N.W.	
18	48	D°	D°	53	Clouds. High. D° Froft	
19	42	D°	D°	50	Clear. Brifk. D°	
20	46	Cloudy.	N. E.	44	Rain. Snow. D°	
21	44	Clear.	N.W.	58	Clear. Brifk. D°	
22	45	Smoky.	D°	65	Smoky.	D°
23	46	D°	D°	70	D°	S.
24	48	D°	D°	59	D°	D°
—						
26	58	Rain. Thunder.	D°	58	Rained.	N.W.
27	54	Cloudy.	D°	60	Clear.	S.
28	52	Clear.	D°	59	Flying clouds. Eaftw. Froft	
29	48	D°	D°	53	Smoky.	S.W.
30	54	Smoky.	S. W.	73	D°	D°

M A Y.

	A.M. ☿	Wind and Weather.		P.M. ☿	Wind and Weather.		
1	66	Cloudy.	S.W.	66	Rained.		N.W.
2	54	Drizzle	N.W.	54	D°		N.E.
3	46	Cloudy	Eaftwardly	53	Drizzle		
4	50	Drizzle		56	Cloudy.		S.W.
5	56	Clear.	N.W.	67	Clear.	Brifk gale.	N.W.
6	55	Clouds.	S.W.	75	D°	D°	D°
7	72	D° Brifk gale.	D°	76	D°	D°	D°
8	60	D°	N.W	67	Cloudy.		D°
9	62	D°	E.	67	D°		E.
11	65	Rained.	D°	63	Rained.		N.E.
12	59	D°	D°	63	Clouds.	Thunder.	E.
13	60	Drizzle		68	D°	D°	D°
14	64	D° Thunder.	E.	67	Rained.	D°	D°
15	65	Rained. D°	D°	69	D°	D°	D°
16	64	Clouds. D°	D°	72	D°	D°	S.E.
17	68	Rain. D°	D°	66	D°		D°
18	56	Clear.	N.W.	67	Fair.		N.W.
19	61	D°	S.E.	73	Clouds.		S.E.
20	65	Clouds.	D°	80	D°		D°
21	72	D°	D°	83	D°	Lightening	D°
22	73	Clear.	N.W.	82	Clear.		N.W.
23	70	D° Brifk gale.	D°	84	D°	Brifk gale.	D°
24	73	D° D°	D°	85	at 3 o'clock, 80° at 8 o'clock th. light. rain. High N.W		
25	71	Clouds.	D°	74	Thund. Rain.		E.
26	70	D°	E.	72	D°	D°	Lightning
27	67	Cloudy		70	Rained. Thunder.		N.E.
28	63	Drizzle.	N E.	52	D°	D°	N.W.
29	45	Clear.	N.W.	60	Clear.	Brifk gale.	D°
30	52	Clouds. Brifk gale.	D°	63	D°	D°	D°
31	51	Clear. D°	D°	71	D°	D°	D°

J U N E.

J U N E.

	A.M ☿	Wind and Weather.		P.M. ☿	Wind and Weather.
1	63	Clouds. Briſk gale. S. E.		76	Clouds. S. E
2	70	D° L°		70	Rain. Thunder.
3	60	Clear. S.W.		71	D° D° Lightn. S. E.
4	67	Cloudy. D°		70	D° D° D° S.W.
5	65	Clear. N.W.		70	Clouds. N.W.
6	65	Clouds. D°		69	Thund. lightn. rain. D°
7	65	D° Briſk gale. D°		64	Clouds. Briſk gale. D°
8	60	D° D°		63	D° S. E.
9	60	D° Briſk gale. N E.		66	D° D° N. E.
10	62	Rained. High. D°		61	Rain'd. High. D°
11	57	Drizzle D°		70	Clear. Briſk gale. N.W.
12	62	Clear. Briſk gale. N.W.		71	D° D°
13	63	D° D°		74	Rained a little. S.
14	65	D° D°		76	Clouds. N.W.
15	68	Cloudy. E.		75	D° E.
16	64	Rained. D°		75	Cloudy D°
17	56	Fog. S.		69	Clear. S.
18	63	Clear. S. E.		75	Clouds. D°
19	67	D° S.		75	D° Thunder. D°
20	72	Drizzle		78	D° E.
21	73	Cloudy. S.		78	D° S. E.
					The *Via Lactea* N. & S. about 45° in the higheſt part from the eaſtern horizon.
22	74	Cloudy. S. E.		77	Rained. S. E.
23	75	Rained. D°		75	D° Thunder and lightn. Briſk gale. S. E.
24	69	Clear. Briſk gale. N.W.		78	Clear. S.W.
25	69	D° S. E.		79	D° S. E.
26	74	Clouds. D°		80	D° D°
27	76	D° S.		87	Rain, thund. lightn. N. E.
28	75	Drizzle E.		67	Drizzle E.
29	67	D° D°		73	D° S. E.
30	71	Cloudy. S. E.		78	Thunder and rain

J U L Y.

J U L Y.

	A.M. ☿	Wind and Weather.		P.M. ♂	Wind and Weather.	
1	68	Clouds.	S.E.	77	Clear.	N.E.
2	72	D°	D°	79	Clouds.	S.E.
3	75	D°	D°	80	Thunder, lightn. rain.	
4	74	Rained.	N.E.	71	Clear.	N.W.
5	63	Clear.	N.W.	74	D°	S.W.
6	68	Rained. Calm.		72	D°	
7	61	D°	D°	76	D°	N.E.
8	71	Cloudy.		65	Cloudy. Calm.	
9		Clear.		83	Clear.	S.E.
10	74	Cloudy.	S.E.	82	Rain, thunder, lightning.	
11	76	Fog		87	Fair.	S.W.
12		Clouds		75	Rain, thund. lightn.	N.E.
13	70	D°	N.W.	76	Clear.	N.W.
14	64	Cloudy.	D°		D°	D°
15	71	D°	S.E.	84	Thund. lightn. rain.	S.E.
16	74	Clear.	S.W.	82	D° D° D° high.	N.W.
17	74	D°	N.W.	82	Fair. D°	D°
18	77	Clouds.	D°	78	Th. and rain. Northwardly	
19	72	Cloudy.	N.E.	74	D° D° Lightn.	N.E.
20	71	Thunder and rain		78	Clouds	
21	71	Clear		85	Clear	
22	76	Clouds.	Eastwardly	85	Thund. light. rain.	S.E.
23	75	Cloudy.	S.E.	84	D° D° D° D°	
24	82	Drizzle.	N.E.	68	Drizzle.	N.E.
25	63	Fair	D°	73	Fair. Brisk gale. D°	
26	63	D°	D°	71	D° D° D°	
27	62	D°	E.	73	Cloudy.	D°
28	68	D°	N.W.	79	Fair.	N.W.
29	71	D°	D°	70	D°	S.E.
30	61	Clouds.	E.	80	Thund. lightn. rain.	E.
31	74	Rained.	N.W.	73	Drizzle.	D°

AUGUST.

A U G U S T.

•	A.M ☿	Wind and Weather.		P M. ☿	Wind and Weather.	
1	64	Fair. Brisk gale.	N. E.	72	Fair.	S. E.
2	67	Cloudy.	D°	73	D°	D°
3	6~	Rained. Calm.		68	Cloudy.	D°
4	6c	D°	N. E.	66	Rained.	N. E.
5	63	Fair. Calm.		73	Fair.	S. E.
6	63	D°		77	D°	
7	72	D°	S. E.	79	Clouds.	S. E.
8	72	Rained		77	Cloudy	
9	73	D°		73	Rained	
10	74	Rained.		78	Cloudy	
11	75	Fair.	S E.	83	Fair. Eastwardly	
12	75	D° Brisk gale.	S.W.	80	Rained. High. N.W.	
13	74	Clouds		78	Cloudy. Thunder. S. E.	
14	72	Fair		83	at 2 o'clock, 75° at 8 o'clock hard thunder, lightn. and rain. High wind. N. E.	
15	72	Cloudy		79	Thun. lightn. & rain. N.E.	
16	75	D°		81	Cloudy. Lightening.	
17	76	Clouds		83	Thunder. D°	
18	76	D°		8_	D° D° Rain	
19	75	D°		82	Cloudy	
20	76	Heavy rain		77	Thund. lightn. rain. E.	
21	74	Fog		76	Fair	
22	76	Clear		87	Clear. S.W.	
23	79	D°		80	D° Westwardly	
24	80	D°		80	D° D°	
25	75	D°		88	D° E.	
26	79	D°		80	D°	
27	75	D°	N.N.W.	85	D°	
29	70	Cloudy		83	D°	
30	75	D°		87	D°	
31	78	Clear			D°	

A Ther-

XII. *A Thermometrical Account of the Weather, for Three Years, beginning* September *1754. as observed in* Maryland. *By Mr.* Richard Brooke. *Communicated by Mr.* H. Baker, *F. R. S.*

The highest and lowest State of the Mercury in each Month is here given, and other Observations are occasionally intermixed.

[Read March 8, 1759.]

1754.

	highest	lowest	
Sept.	8o		Winds for the most part easterly, and very rainy.
Oct.	8o	34	Winds easterly, and much rain in the beginning: latter end fair.
Nov.	67	23	Winds variable. Snow and rain.
Dec.	5o		Much rain.

1755.

	highest	lowest	
Jan.	69	23	Much rain. Snow on the 1st day.
Feb.	64	14	Much snow.
Mar.		24	Much rain.
Apr.		40	On the 16th it snowed as hard as ever I knew. Cleared up at two o'clock P. M. all dissolved before night. Not one shower of rain this month. Wind easterly till the 14th; afterwards mostly westward.

May

	Mercury.	
	Higheſt	Loweſt

May | 57 | 47 | Extremely dry : ſeldom any clouds : no rain. Every vegetable almoſt burnt up : ſtrawberry-leaves, green plantain, and others, ſo criſp as to crumble. In this month many black cattle died for want of food.

June | 90 | 70 | Seaſonable weather.

July | 93 | 60 | Very dry.

Aug. | 90 | 61 | Very dry.

Sept. | 93 | 45 | Very dry.

Oct. | 75 | 36 | Seaſonable weather towards the end of this month. This was the drieſt ſummer and autumn ever remembered. Many ſprings dried up, that ran briſk before. My ſpring, a remarkable good one, ran very ſlow, and the water was unpleaſant.

Nov. | 65 | 29 | On Tueſday the 18th I felt three ſhocks of an earthquake about eight minutes before four in the morning. The firſt was ſevereſt : it ſhook the houſe very much, and waked me. The ſecond was leſs, and the third leaſt of all. They ſucceeded each other at about one minute's diſtance, and were felt all over the continent.

Dec. | 71 | 15 | On the 16th there was a briſk ſoutherly wind ; the mercury about noon at 71°. At four P. M. ☿ at 69°: at five o'clock the wind came about to N. W. blew exceſſively hard, and did great damage

in

in the country. A prodigious quantity of rain fell: it cleared up at fix o' clock; but the wind continued blowing hard all night. At eight o'clock the ☿ was at 43° at seven next morning at 26°, at nine at 24°½, and the morning following, *viz.* the 18th, the mercury was at 15°.

1756.

	Morn.		
	☿	☿ lowest	
Jan.	73	15	
Feb.	70	27	
Mar.	—	—	Not observed, being hindered by business.
Apr.	23	29	Seasonable weather.
May	31	48	Seasonable weather.
June	56	44	Plenty of rain : the season much colder

than usual, ☿ standing frequently between 60° and 70°. On the 22d in the morning a black cloud came from the northward, soon overspread the hemisphere, and threatned much wind and rain ; but soon blew over without much wind or rain. The sun shone clear, and the weather calm, till towards noon, when clouds collected towards the north and north-weft. About three P. M. there was the moft threatning appearance I ever beheld : the clouds in some places of a deep green ; in others, of a footy black. At 45 min. paft three it began to rain and blow, attended with remarkable severe thunder ; but as the thunder

thunder ftopped the clock, I cannot fay how long it lafted; but fuppofe near half an hour; in which time the moft rain fell I ever faw. The wind did incredible damage in feveral parts of the country. In St. Mary's county, it is faid, 200 houfes were blown down, and many people killed. In every county in Maryland much damage was done by this guft, which was the moft general ever remembered. It was all over New York, the Jerfeys, Penfylvania, Maryland, and Virginia, and did much damage every where. How much farther it extended, either northward or fouthward, I have not heard.

In thefe two laft months an epidemical fpotted fever was common in the country: I have not heard it was mortal any where. Thofe, who had it moft fevere, were relieved with what Mr. Collinfon calls *apocinon*. There raged at the fame time an epidemical difeafe amongft the dogs, which deftroyed great numbers in all the northern plantations. It came from thence to the eaftern fhore in Maryland, where it killed moft of the dogs. It now rages amongft our dogs, and fcarce any recover. They are firft feized with a fhort cough, and a ftoppage in the nofe, fo that they are obliged to breathe thro' the mouth. In four, five,

| Mercury. | |
Highest.	Lowest.

five, or fix days after, they have a large difcharge thro' the noftrils of a thick fetid matter, and a plentiful ferous difcharge from their eyes. Now their ftomach fails, or rather they are afraid to eat; for every attempt makes them cough violently, and feems to give them great pain. Some die within two days after this difcharge; fome live a week, or longer: thefe have had food forced into their ftomachs: but none ever recover, that I have heard of.

July 93 69 Seafonable weather, and the moft plentiful appearance of corn and tobacco I ever faw. The wheat was got in laft month: it is fuppofed there will be the moft of any year fince the fettlement of this country.

Aug. 93 68 Very dry. The difeafe amongft dogs continues, tho' lefs violent: many have their limbs feized with a paralyfis: thefe all recover.

Sept. 92 60 The difeafe continues amongft the dogs. This month I faw a tame fox very ill with this diforder. I gave him a dofe of a valuable powder, with which I have done much good; and for the knowlege of which I was obliged to my worthy friend Dr. Parfons, when I was laft in England. I have known this powder cure dogs; which made me give it to this fox: but he died in

three

| Mercury. | |
| Higheſt. | Loweſt. |

three minutes after; which I attribute to the punch in which I gave it. This is the hotteſt and drieſt ſummer ever known in Maryland There are great crops of corn and tobacco made; but, through the extreme dryneſs of the weather, the latter crops of neither will come to perfection. Many ſprings are dried up, that were ever current before. Putrid bilious fevers are now very frequent in the country, and have raged for theſe ſix weeks paſt. So likewiſe has an hepatic dyſentery, which, I have been informed (for it has not come within my own prac-tice), has oftentimes been ſo malignant, as not to yield to any medicines that could be thought of.

Oct. 90 29 The weather ſeaſonable. The dyſente-ries, that have been ſo fatal in many parts of the province, have reached my neighbourhood. The bilious fe-ver now is, and for ſome time paſt has been, very common in this coun-try. As the patients under my care had frequent large bilious ſtools (after the ſtimulus on the inteſtines was removed by opiates, and they voided no blood), imagining the fever and the flux owing to the ſame cauſe, only affecting different parts, I gave Dr. Parſons's powders, which I always uſed with ſucceſs, and my patients

	Mercury.	
	Higheſt.	Loweſt.
Nov.	75	27

got well. The method I found fuc-
cefsful, after trying many others, was
to give opiates, at proper intervals, till
the purging and bloody ſtools ceaſed :
then four or five doſes of Dr. Parſons's
powders : afterwards a few doſes of
aſtringents, which never failed of cur-
ing. The opiate I uſed was *pil. mat-
thæi* ; which I ſuppoſed beſt in this
putrid bilious complaint, becauſe of
the ſoap in its compoſition. It is re-
markable, that the frequent repetition
of the ſtrongeſt aſtringents never de-
creaſed the quantity or number of the
ſtools, till Dr. Parſons's powders were
given. Theſe valuable powders, which
that ingenious and benevolent gentle-
man communicated to me, when I
was at London, have been of incre-
dible ſervice in the plantations. I am
perſuaded he will readily inform any
practitioner what they are ; and that
nothing is wanting to bring them into
general uſe, in all bilious and putrid
fevers, but a thorough knowledge of
their extraordinary efficacy.

The hepatic flux ſpreads very much in
my neighbourhood. On the 22d I
was called to a family, where the
miſtreſs and maid were both down
with this diſorder, and appeared to
have it very much alike. They were
much griped, and purged more than

20 times in 24 hours: fometimes blood in a large proportion was mixed with their ftools; but the moft troublefome fymptom was a violent *tenefmus*. The miftrefs took two dofes of Dr. Parfons's powder, and the maid as many of Dr. James's. At night the miftrefs took an anodyne draught, and the maid *gr*. 8 *pilulæ matthæi*. The next day they took fome aftringent powders, at night their anodyne, and the day following both were well. The fmall-pox and fwine-pox are now about the country.

	Mercury.	
	Higheft.	Loweft.
Dec.	63	13

People in general very healthy. The fmall-pox fpreads but flowly, and is very favourable.

1757.

Jan.	65	10

Many fudden alterations, as to heat and cold, have been in this month: but the moft remarkable I have ever obferved, was the laft day of this month, when the ☿ was up at 65°, and the next day, Feb. 1. when it was down at 28°, about the fame hour in the day.

The people of Maryland were afflicted laft year with fcarce any complaints which were not attended more or lefs with bilious fymptoms; and as the year advanced, the weather being unufually hot, fuch fymptoms grew

more

more violent, and the bile more pu-
trefcent. A bare ftate of the many
odd cafes that came under my care
would fill feveral fheets of paper ; but
as my own was very particular, I fhall
only mention that. I was taken on the
24th Dec. laft with a flight fever and
fore tnroat, which continued three
days : then the fever and fore throat
left me ; but for three nights after-
wards I felt more uneafinefs than I
ever before was fenfible of, tho' quite
free from pain or fever. I waked, I
fuppofe, at leaft an hundred times
each of thefe nights. If I tried to lie
on either fide, I always turned in-
fenfibly on my back. When afleep,
my imagination was filled with the
moft frightful ideas that ever difturb-
ed an human mind ; but I could re-
member none of them when I awoke.
I conftantly loft my breath when
afleep ; and was waked by an hideous
whooping noife, like that of a child in
a chin-cough. I prefently fell afleep
again, and the fame horrid fcene was
re-acted. The feventh night from my
firft attack was attended with a new
train of fymptoms. About three o'
clock in the morning I was afflicted
with a moft excruciating pain about
the fifth or fixth vertebra of the back,
like the boring of an augre. I was
forced to quit the bed, and, to my
great

Mercury.	
Higheft.	Loweft.

great furprize, was eafy almoft as foon as I got on my cloaths : but it returned as foon as I attempted to lie down again. This lafted 27 days, ever free from pain while up, and always taken about feven hours after lying down, either by night or day. Neither the Indian bath, blifters, or any thing I could think of, did relieve me : but getting up always eafed me. It was plainly a fpafm of the mufcle trapizeus ; for if I rubbed my back ever fo flightly, the pain would inftantly remove to the mufcles of the fternum, *et vice verfa* ; yet I could never feel it in my fide. Whether it was leaving me or not, I cannot fay : but I feemed to find great relief by drinking punch, into which Goa ftone had been plentifully grated. It is very ftrange, that during the whole time I had no fever, or loft my appetite, tho' I grew very weak.

Feb. 67 5 It rained almoft every day this month : and at this time there prevailed a diforder among the Negroes, which, I believe, was peculiar to them ; for I never heard of any white perfon's having it. They were taken with pains in their heads, necks, fhoulders, breafts, or bellies : it feldom continued long in a place, till it got to the thigh, where the complaint would

form

form into a very hard and fenfible tumour, generally in the triceps mufcle. Emollient plaifters would commonly remove the tumour in two days time. The morbific matter would give exquifite pain in its defcent down the thigh, and would collect again into a tumour, either in the ham, or in the calf of the leg. The fame cataplafms continued would repel the tumour, and the patient would get well ufually in about a fortnight. There was an high fever during the time. I never knew but one fuppurate, tho' I have feen many in this complaint : this happened to a Negro boy about 15 years old : here the matter defcended to the foot, which was very troublefome to heal.

Mar.|65|30 A vaft deal of rain fell this month.

Apr.|67|35 The wetteft and coldeft April within man's memory. Impetigenous diforders very common both in Maryland and Virginia, and fome very obftinate.

May |88|48 Seafonable and healthy.

June|9c|72 An uncommon wet month. The hard rains beat off the flour, or *farina fecundans*, of the wheat ; fo that very little of that grain was made this year.

July |90|64 Much rain. It is very remarkable, that many people, this month and the laft,

in

in different parts of the country, were troubled with imposthumations under the arm, all in the *axilla dextra*. They maturated, and healed up pretty eafily. Whether this has any affinity to the difpofition of the prefent and preceding year, I cannot fay : I before obferved, that almoft every patient, in whatever diforder, had more or lefs of bilious fymptoms. I have feen more inflammations of the neck of the bladder this month than all my life before : They were cured by antiphlogiftic medicines.

Mercury.		
Highest.	Lowest.	
Aug.	90	67	Much rain, and thick foggy weather.
Sept.	88	47	Very wet.

Much rain, and thick foggy weather.

Very wet. Within thefe two months I have feen five perfons, and have heard of many more, who were taken with a violent pain in the *os fronits*, on the left fide. The pain foon fell into the Eye on the fame fide, and occafioned a dimnefs : but this and the pain were foon removed by an epifpaftic behind the ear, if applied early. I was called to a Negro wench, who had had the complaint fo long, that fhe was totally blind of both eyes ; which appeared, as in the *gutta ferena*, without any inflammation, or vifible defect ; but extremely painful. In her likewife the left eye was firft affected, and much the moft difficult of cure. Cauftics behind the ears, and vitriolic colly-

	Mercury.	
	Higheſt.	Loweſt.

collyriums, cured her : but three cauſtics were put behind the left ear, before that eye was well.

Oct. | 67 | 43 | Very wet. Many horſes died this month of a peſtilential fever. They had the ſymptoms Markham deſcribes in his chapter of Peſtilential. Hen-dung infuſed in ſtale urine (which Markham recommends) was found ſerviceable. Many people were ruined by the loſs of all their horſes : but none died that had plenty of the juice of rue. It is remarkable that no horſes had this diſtemper, but thoſe on the ſalts of the different rivers. Mr. Pollard, Edmunſon, a gentleman on the eaſtern ſhore of Cheſſapeak, told me, he loſt almoſt every horſe by this diſorder at his home plantation (the water there is ſalt), and not one in a foreſt paſture about a mile off.

Nov. | 65 | 33 | The diſeaſe among horſes is over. On the 10th day of this month there was as ſevere a guſt of thunder and lightning, as is common in July or Auguſt. Several horſes, cattle, &c. were killed in different parts. There were the moſt luminous coruſcations I ever ſaw; the whole hemiſphere as it were in a blaze.

Dec. | 68 | 28 | Very variable weather : many high winds, and much rain.

XIII. A

XIII. *A Letter from Mr.* Benjamin Wilſon,
F. R. S. to the Rev. Tho. Birch, *D. D.*
Secret. R. S.

Dear Sir,

THE incloſed letter contains ſome
electrical experiments and obſerva-
tions, which ſeem to merit the attention of the
Royal Society. I wiſh therefore you would lay it
before them ; and at the ſame time ſignify, that I
have ſeen all the experiments carefully made, and
that the ſeveral facts therein contained are faithfully
related. I am,

SIR,

Your moſt obedient humble Servant,

Queen-ſtreet, London,
22 March, 1759. B. Wilſon.

A Letter from Edward Delaval, *M. A. and
Fellow of* Pembroke-Hall, Cambridge, *to
Mr.* Benjamin Wilſon, *F. R. S. containing
ſome Electrical Experiments and Obſerva-
tions.*

SIR,

I Send you a few electrical experiments
and obſervations ; and deſire your opi-
nion, how well they eſtabliſh a convertibility, I be-
lieve hitherto unnoticed in many ſubſtances, from
conductors into non-conductors of the electric fluid.

M 2 I have

I have filled several small glass tubes with the dry powders of calcined metals, *viz.* cerufs, lead ashes, minium, calx of antimony, &c. Into each end of every tube I put a piece of iron wire, which communicated with the calx, and fastened them with wax: so that the electric fluid, not being able to escape by means of the glass, must either pass thro' the calx, or not at all. Upon hanging one of the wires, bent for the purpose, to the electrified bar, and holding the other in my hand, I observed that no electric matter did pass the calx, the snaps issuing all the while from the bar, or from that wire which was in contact with the bar *.

Animal and vegetable solids also, when reduced to ashes, and interposed in the same manner between two pieces of wire, do, I find, as effectually intercept the electric stream, as the metallic calces.

From these experiments you see, that animal, vegetable, and metallic bodies, tho' such known conductors of the electric fluid while in their intire state, are easily changed into resisters or non-conductors of it.

I was led to attempt this change from its having been observed, that dry mould would not conduct the electric fluid : and from thence I suspected, that one class of the non-conductors must owe its property to an electrical virtue that would be found to reside in the calx, or earth of the chymists, after it is divested of the unctuous inflammable matter, which

* Since I wrote this letter, I have been informed, that part of this first experiment, relating to metallic calces, has been made before by Dr. Watson. See the note Phil. Transf. vol. xlv. p. 107.

consti-

conftitutes another of the chymical principles called fulphur; in like manner as this fulphur is conftantly found highly electrical in all bodies where it abounds in a folid form, *viz.* refins, wax, &c.

Thefe experiments appear to verify my fuppofition: for all the above-mentioned fubftances, which were thus changed into non-conductors, confift either wholly, or in a great meafure, of earth freed from the unctuous inflammable particles; the metals not being calcineable without a degree of heat that muft diffipate all their fulphur, as is evident from their not being reducible again into their metallic form without the admixture of fome unctuous matter; and the fame diffipation of their fulphur muft take place in the animal and vegetable fubftances, before they become white afhes.

I fhall not at prefent attempt an account, why bodies confifting of either of thefe fubftances feparately are electric, tho' it appears to me deducible from fome doctrines of Sir Ifaac Newton; but only propofe a thought concerning the reafon why thefe two principles, calx and fulphur, which are known to unite in the compofition of almoft all bodies, fhould, notwithftanding they are electric when feparate from each other, be yet found nonelectric when united in one body.

It muft be remembred, that there is a remarkable and well-known oppofition in the electrical effects of thefe two claffes; the earthy one (as glafs and ftones) electrifying *plus*, and the fulphureous one *minus*. Does it not feem then a thing to be expected, in a body compounded of both, that the oppofite powers of thefe ingredients fhould counterballance

and

and deftroy the effects of each other, and the body in which the pofitive and negative ones equally prevail, become neutral, or non-electric?

I have not fcrupled to rank thofe known pofitive electrics, glafs and tranfparent ftones, under that clafs of bodies which confifts of calx or earth; becaufe all vitrifications muft proceed from previous calcinations, and all calces may be vitrified in the focus of large burning-glaffes. The tranfparent ftones alfo confift of little more than pure earth, free of the leaft mixture of oil, if we may judge of others by the chymical refolution of cryftal.

There is another procefs, natural and without fire, which is fuppofed to deftroy the fulphureous fubftance of metals, *viz.* when they are corroded, and moulder in the open air. Accordingly, with the fame apparatus in which I tried the calcinations by fire, I examined the common ruft of iron, and flake-white, which is the ruft of lead, and find them equally converted into non-conductors in the open air.

That this change, in metals particularly, is not owing to, or promoted by, the circumftance of mere pulverization, is evident, not only becaufe the above-mentioned calces are equally ftrong electrics when formed into hard maffes with a thin pafte of flour and water, and afterwards dried, but moft clearly becaufe the fineft filings or powders of metals conduct as readily as the intire fubftances do. I have glafs tubes armed as above, and filled with the preparations called powder of tin, &c. which conduct as well as a wire when it is not difcontinued.

But notwithftanding this change will not fucceed

9   in

in metallic fubftances upon mere pulverization, yet it feems to follow in moft other hard bodies.

Having dried a piece of Portland ftone, I found it conducted perfectly well; but upon powdering, and fealing it up in one of the tubes with the wire ends, as above, it became a perfect refifter, or non-conductor, like the metallic calces.

I have tried the fame experiment on a variety of other bodies, particularly gum arabic and allum; and have reafon to believe it will fucceed in all bodies that can be pulverized in the mortar.

Thefe laft experiments feem to confirm Sir Ifaac Newton's doctrine of a *medium* furrounding all bodies, which you have applied to the folution of electric phænomena, and are very analogous to the experiments you made with a *chain*, in order to fhew that the refiftance to the paffage of the electric fluid may be increafed by increafing the number of furfaces.

Another very extraordinary means of making this change in bodies, which abound in calx or earth, is by fire: not by the intenfe one that calcines, but by a moderate heat; their moft perfect refiftance, or non-conducting property, being when their heat is juft tolerable to our hands.

I have fome of the fame Portland ftone, wrought into plates nearly as thin as window-glafs, which I heat to a proper degree, and then coat on both fides with metal, in order to make the Leyden experiment. When the ftone is hot enough to finge paper, it conducts as perfectly as when cold; but on cooling a little, it begins not to conduct, and affords fmall fhocks, which gradually increafe in ftrength for about ten minutes; at which time it is about its moft per-

fect

fect ftate, and remains fo near a quarter of an hour: after that time the fhocks gradually decreafe as the ftone grows cooler, till at laft they quite ceafe, and it returns to its conducting ftate again : but this ftate appears before the ftone is quite cold.

Experiments of this kind fucceed in all bodies abounding in calx or earth, as ftones, dried clay, wood when rotten or burnt in the fire till the furface becomes black.

Among other fubftances, I tried a common tobacco-pipe, part of which near the middle I heated to a proper degree, and then applied one end of it to the electrified bar, while the other was held in the hand; and I obferved that the electric fluid paffed no farther along the pipe than to the heated part.

To thefe changes brought about with fudden violence, I muft add the univerfal change going on in all animal and vegetable folids, as they are growing dry. Not only their afhes refift the paffage of the electric fluid, but they of themfelves arrive at this ftate while yet hard and intire; and that much fooner than one would imagine; for I have bones and hard wood that perfectly refift the paffage, tho' yet capable of yielding a bright flame, but fcarce a vifible fmoke : fo that befides an evaporation of their moifture, but a partial progrefs can have been made in the difcharge of their fulphur.

I fubmit to your judgment, how much this convertibility may contribute to a farther knowlege of the laws of electricity. I am, Sir,

Your moft humble Servant,

Old Palace-Yard,
March 15, 1759.

Edward Delaval.

XIV. *An*

XIV. *An Account of the Case of* William
Carey, *aged Nineteen, whose Tendons and
Muscles are turning into Bones. In a
Letter to the Right Honourable the Lord*
Cadogan, *F. R. S. from the Rev.* William
Henry, *D. D. F. R. S.*

Castle-Caldwell, near Enniskillen,
March 1. 1759.

My Lord,

Read Mar. 22, **HAVING** come hither with the
1759. Earl of Shelburne, on a visit to Sir
James Caldwell and his Lady, we met with a
young man, whose case is of so extraordinary a na-
ture, that we thought it might be of public utility
to examine into it strictly, and transmit it to your
Lordship.

A great part of his body is, within the space of
two years, ossified ; and the ossification is continually
seizing more of the muscles.

I have in the case barely set down the facts, with-
out any reasoning thereon. But, so far as I can con-
jecture, there seems first to ooze out of the joints a
kind of jelly, which by degrees grows hard, fills up
gradually the smaller vessels, and concretes into bone.
If it goes on, I believe within a very few years the
man, if he can live, will be completely ossified.
Perhaps it may be of some benefit to mankind to
have his case made known to the Royal Society, or
to the College of Physicians. Your Lordship's judg-
ment will determine best how proper this may be.

N My

My Lord Shelburne, and all his family, join in all poſſible reſpects with

Your Lordſhip's

Moſt obedient,

and moſt humble Servant,

William Henry.

The CASE *of* WILLIAM CAREY.

HE was born in an iſland in Lough Melvill, a large lake in the northern point of the county of Leitrim in Ireland, and has continued therein, or in the adjacent lands, ever ſince.

He was bred up to work as a labourer, and continued in very good health from his birth until two years ago. About that time he firſt felt an unuſual pain in his right wriſt, which in Auguſt 1757 began to ſwell: this obliged him to ceaſe from his uſual labour. In the ſpace of a month more, this ſwelling grew into an hardneſs, like to a bony ſubſtance; and continually ſhooting on, in December reached up as far as the elbow; all the muſcles continually growing into a bony ſubſtance, and dilating ſo, that his wriſt and arm are twice as thick and broad as in the beginning. About the ſpace of a week after the pain began in his right wriſt, he was ſeized with the like pain and ſwelling in the left wriſt. This has proceeded in all reſpects in the ſame manner as in his right arm. The whole ſubſtance of each arm, from the elbow down to the wriſts, feels as if it were one ſolid bone.

The

The offification is fhooting downwards into the fingers, and upwards into the elbows ; fo as already to prevent the bending of the fingers or elbow of the ᐧleft arm. It has likewife fhot upwards, fo as to feize the great mufcles of each arm between the elbows and fhoulders.

The continual pain and dilatation of the arms occafioned a burfting of the fkin and flefhy parts about each elbow in November 1758 ; out of which oozed a thin yellowifh humour, with a little digefted *pus.* Some of thefe breaches have healed up of themfelves. One fmall orifice in each elbow ftill continues to run.

In March 1758. he was feized with the like pain and fwelling in his right ancle, whence fuch another bony fubftance foon grew as in his arms. This bony fubftance has fhot up from his ancle, both in the inward and outward fide of the right leg, half way up to the knee ; and the like bony fubftance has, in the inward fide, fhot downward from the pan of the knee eight inches along the fhin-bone, and is daily increafing ; fo that he walks with much pain and difficulty, and after refting in his walk, grows very lame. This perfon is of a very thin habit of body, and in fize five feet nine inches ; fomewhat inclined to an hectic, tho' he has no cough.

The above-mentioned William Carey was infpected, and clofely examined, as to all the above particulars, at Caftle-Caldwell, in the County of Fermanagh, this 1ft Day of March 1759, by us

This is exactly my cafe.
  William Carey.

SHELBURNE.
JA. CALDWELL.
WILLᴹ HENRY.

XV. *A further Account of the same Case:
in a Letter to the Right Honourable the
Lord* Cadogan, *F. R. S. from the Rev.*
William Henry, *D. D. F. R. S.*

My Lord,

Read June 14, 1759. I HAVE now standing by me William Carey, the young man, of the ossification of whose limbs I had the honour formerly to acquaint your Lordship: and now, in obedience to your commands, give an account of his case since that time.

I had him sent up in March last to Mercer's Hospital in this city. After examining his case, the physicians and surgeons concluded, that the only probable chance to prevent the progress of the ossification, and to remove the evil already effected, was putting him into a mercurial course. This they tried; and after some slighter mercurial medicines, they, in the latter end of April, laid him down in a salivation, thro' which he passed with safety.

This dried up the running sores at his elbows, occasioned by the bursting of the skin, thro' the ossification. Some lighter callus, which was shooting into bones, seems to be softened: in consequence of which he can move his elbows, and the joints of his fingers, with more ease; and he has a little more clearness and vivacity in his countenance: but none of the ossified parts are reduced, nor is there any appearance of their reduction; and he still continues to wear an hectic look. To reduce the ossified parts,
they

they have applied to them mercurial plaifters; the effect of which time will fhew.

As he is now difcharged out of the hofpital, they have directed him to bathe continually in the ocean, which happens to be very convenient to his habitation; and have directed him to anoint his limbs with the foapy juice of the *quercus marina*, which lies in plenty along the fhore. I fhall attend to the event of this procefs, and fend your Lordfhip a particular account of it.

I am, with all regard,

Your Lordfhip's much obliged,

and moft obedient humble Servant,

Dublin,
May 24. 1759.

William Henry.

XVI. *An Account of the Comet feen in* May 1759. *By* J. Bevis, *M. B.*

Read May 3, 1759.
I Had acquainted fome of my friends, that it was my opinion a comet would hardly arife above our horizon of London Sunday April the 29th; but that probably we might fce one April 30th.

Sunday was very clear; but I could not fee it.

Monday, not fo clear: however, I faw what I took for the comet thro' the fmoke of the town, about ten, near the horizon.

Tuefday, May 1. faw it very plainly, at Mr. Short's, from nine to eleven. We compared it by

means

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means of the equatorial inftrument with α, β, and ε *corvi*; whence its right afcenfion, at 8 h. 45 m. mean time, comes out 159° 55′ 9″, and its fouth declination 25° 52′ 14″.

Wednefday, May 2d, obferved it again at Mr. Siffon's in the Strand, with a fector of 5 feet radius, and compared it with β *corvi*; whence, at 9 h. 6 m. mean time, its right afcenfion 158° 47′ 37″, and its fouth declination 22° 19′ 23″. The increafing moon had now much weakened the light of the comet, fo that the tail and nucleus could not be diftinguifhed as laft night.

I think I may now venture to pronounce this to be the fame as the comet of 1682; and am about making out its future track. If I prefume rightly, it will in a fhort time become in a manner ftationary, but diminifh very faft both in fize and light; the earth and it both receding from each other almoft in a right line. It is at this time about four times nearer to the earth than the fun is.

May 3. 1759.   J. Bevis.

An Account of the fame Comet: By Nicolas Munckley, *Efq*; *Communicated by* Nicolas Munckley, *M. D. F. R. S.*

THE firft certain view I had of any appearance, which could be the expected comet, was on the evening of the 30th of April, about S. S. W. a little lower than the middle of Hydra. But I did not attempt to determine its

place

place with any precision, till a second observation had made me more fully satisfied of its being the phænomenon I wished it.

The following evening, May 1st, its place about ten o'clock (as well as I was able to fix it with no better assistance than a common globe, a quadrant, and Senex's planisphere) was, right ascension about 160 deg. declination a little more than 25 deg. S. in a part of the heavens not formed into any constellation, about 9 deg. below the star in *Crater*, mark'd in Bayer's catalogue α, and nearest to χ in *Hydra*; which last star was about 3 deg. to the east of the comet. It is a luminous appearance, very evident to the naked eye (notwithstanding the light of the moon, within two or three days of her quadrature), yet rather dim than splendid; large, but very ill defined. The telescope, at the same time it magnifies it, seems to render it more obscure. The nucleus appears to me to be rather surrounded with a circular haziness, than to have a tail in any particular direction, especially as seen thro' a telescope.

Hampstead,
May 1. 1759.   Nicolas Munckley.

P. S. The 2d of May I saw the comet again very distinctly with the naked eye; but being then in London, without either globe or planisphere, I did not pretend to settle its place.

The cloudiness of the evenings prevented my seeing the comet any more till the 5th and 6th of May: and on these days partly thin clouds, and partly the increasing light of the moon, rendered it much less easily discernible, both by the naked
eye

eye and in the telescope. As the same causes obscured almost all the stars near it, I had great difficulty in fixing its place on the globe. It appears however, now, evidently, to be moving contrary to the order of the signs, and more considerably northwards, *i. e.* slowly retrograde, with a decreasing south latitude.

Hampstead,   N. M.
May 6. 1759.

XVII. *A Catalogue of the* Fifty Plants *from* Chelsea Garden, *presented to the* Royal Society *by the worshipful Company of Apothecaries, for the Year* 1757, *pursuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. & Soc. Reg. nuper* Præses, *by* John Wilmer, *M. D. clariss. Societatis* Pharmaceut. Lond. *Socius, Hort.* Chelsean. *Præfectus & Prælector Botanic.*

Read May 3, ⎫ 1801 A Cinos Syriaca, folio tenuiore,
    1759.   ⎭   capsulis hirsutis. Mor. Hist.
1802 Ægilops Lobelii.
1803 Ambrosia maritima. C. B. 138. Ambrosia quibusdam. J. B. 3. 190.
1804 Arum Zeylanicum humile latifolium, pistillo purp. Miller.
1805 Astragalus caulescens erectus pilosus, floribus spicatis, leguminibus subulatis pilosis, Lin. Sp. Pl. 756.

1806

1806 Cerastium, floribus pentandris, petalis integris,
Loefl. Defc. 26.

1807 Chenopodium foliis lanceolatis carnofis, co-
rymbis dichotamis fpinofis, Lin. Sp. Plant.
221.

1808 Chironia frutefcens capfulifera, Lin. Sp. Plant.
190.

1809 Clethra Linnæi Alnifolia Americana ferrata, flo-
ribus pentapetalis albis, in fpicam difpofitis,
Pluk. Alm. p. 18. tab. 115. fig. 1. Catefby,
vol. 1. p. 66. tab. 66.

1810 Clinopolium orientale origani folio, fl. mini-
mo, T. Cor.

1811 Delphinium nectariis dyphillis, labellis integris,
floribus fubfolitariis, foliis compofitis line-
ari-multipartitis, Hort. Upfal. Delphinium
elatius fubincanum perenne, flor. amplis
azureis, Amman. 132.

1812 Doria Americana lato rigido folio, Boerh. Ind.
Alt. 98. Virga aurea ex Nova York, foliis
fymphiti majoris hirfutis, Schol. Botan. Par.

1813 Gallium album linifolium, Barrel. obf. 99.

1814 Galium caule erecto, foliis quaternis lanceo-
latis trinerviis, Fl. Lap. 60. Gallium album
quadrifolium erectum, Celf. Upfal. 22.

1815 Galeopfis, five Urtica iners, flore luteo, J. B.

1816 Genifta ramis ancipitibus articulatis, foliis ova-
to-lanceolatis, Hort. Cliff. 355.

1817 Geranium Batrachoides Americanum macula-
tum, floribus obfolete purpureis, Hort. Elt.
158.

1818 Geranium pedunculis fubunifloris, foliis quin-
quepartitis acutis, foliolis pinnatifidis, Lin.
Sp. Pl. 685.

1819 Gladiolus foliis linearibus fulcatis, caule bifloro, tubo longiffimo, fegmentis æqualibus, Miller's Dict.

1820 Glycyrrhiza filiquofa, vel Germanica, C. B. P. 352.

1821 Hydrangea, flor. Virgin. 50.

1822 Lamium foliis caulem ambientibus, C. B. 231.

1823 Larix orientalis, fructu rotundiori obtufo, T. 586. Cedrus magna five Libani conifera, J. B. 1. 277.

1824 Lotus maritima lutea filiquofa, folio pingui glabro, Bot. Monfp.

1825 Lychnis faponaria dicta folio convoluto, Gentiana concava, Ger. 253.

1826 Mandragora fructu rotundo, C. B. 169. Off. 300.

1827 Mefpilus fpinofa, pyri folio, H. Leyd. Pyracantha quibufdam. J. B. 151.

1828 Mefpilus foliis lanceolatis ferratis, fpinis robuftioribus, floribus corymbofis, Miller's Ic.

1829 Mefpilus foliis cordato-ovatis, acuminatis, marginibus acute ferratis, ramis fpinofis, Miller's Ic.

1830 Mitella fcapo nudo, Hort. Cliff. 167.

1831 Oenothera foliis radicalibus ovatis, caulinis lanceolatis obtufis, capfulis ovatis fulcatis, Miller's Icons.

1832 Onobrychis major, filiculis echinatis, criftatis, in fpica digeftis, Mor. Hift. 2. 131.
Onobrychis foliis viciæ, fructu echinato, major, C. B. 350.

1833 Orobus foliis conjugatis fubfeffilibus, ftipulatis, dentatis, Hort. Upfal, 220.

Lathy-

Lathyroides erecta, folio ovato acuminato, cæ-
ruleis viciæ floribus, et filiquis Siberica, Am-
man. Ruth. 151. T. 7. F. 2.

.1834 Padus foliis oblongo-ovatis femper virentibus
eglandulofis, Miller's Icons.

Laurocerafus Lufitanica minor Azarero Lufita-
norum, Tourn. Inft. R. H. 628.

1835 Polemonium foliis pinnatis radicibus repta-
tricibus, Flor. Virgin. 22.

1836 Pulmonaria calycibus abbreviatis, foliis lanceo-
latis obtufiufculis, Lin. Sp. Pl. 135.

Pulmonaria non maculofa foliis glabris, Ame-
ricana flore patulo cæruleo, Pluk. Phyt.
Tab. 227. Fig. 6.

1837 Salicornia, T. Cor. 51. Kali geniculatum, Ger.
Em. 535.

1838 Senecio corollis radiantibus, foliis crenatis, in-
fimis cordatis petiolatis, fuperioribus pinna-
tifidis lyratis, Flor. Virgin.

1839 Serratula, C. B. P. 235. Jacea nemorenfis,
quæ Serratula vulgo, T. 444.

1840 Serratula foliis oblongo-ovatis, obtufe dentatis,
caule ramofo patulo, calycibus fubrotundis
mollibus, Miller's Dict.

1841 Spartium ramis oppofitis angulatis, foliis op-
pofitis fubulatis, Lin. Sp. Pl. 708.

1842 Spiræa Africana odorata, foliis pilofis, Com.
rar. 3.

1843 Stramonium Americanum maximum, flore al-
bo, fructu rotundo fpinofo, Tourn.

1844 Tilia foliis molliter hirfutis, viminibus rubris,
fructu tetragono, Ray, Synop. 2. 316.

Tilia hirfuta coryli foliorum æmula, fructu
angulofo, Pluk. Mant. 181.

O 2   1845 Tril-

1845 Trillium flore pedunculato cernuo, Lin. Sp. Pl. 339.

1846 Valeriana montana fubrotundo folio, C. B. P. 165.

1847 Vicia pedunculis multifloris, petiolis polyphillis, foliolis lanceolatis glabris, Hort. Upfal, 219.

1848 Vicia fylvatica multiflora maxima perennis, tetro odore, floribus albentibus, lineis cæruleis ftriatis, Pluk. Alm. 387.

1849 Vinca foliis oblongo-ovatis integerrimis, tubo floris longiffimo, caule ramofo fruticofo, Miller's Iconf.

1850 Xanthium, five Lappa minor, J. B. 3. 572. Lappa minor, five Xanthium Diofcorid. C. B. P. 198.

XVIII. *An experimental Enquiry concerning the natural Powers of Water and Wind to turn Mills, and other Machines, depending on a circular Motion. By Mr. J.* Smeaton, *F. R. S.*

Read May 3, & 10, 1759.

WHAT I have to communicate on this fubject was originally deduced from experiments made on working models, which I look upon as the beft means of obtaining the outlines in mechanical enquiries. But in this cafe it is very neceffary to diftinguifh the circumftances in which a model differs from a machine in large; otherwife a model is more apt to lead us from the
truth

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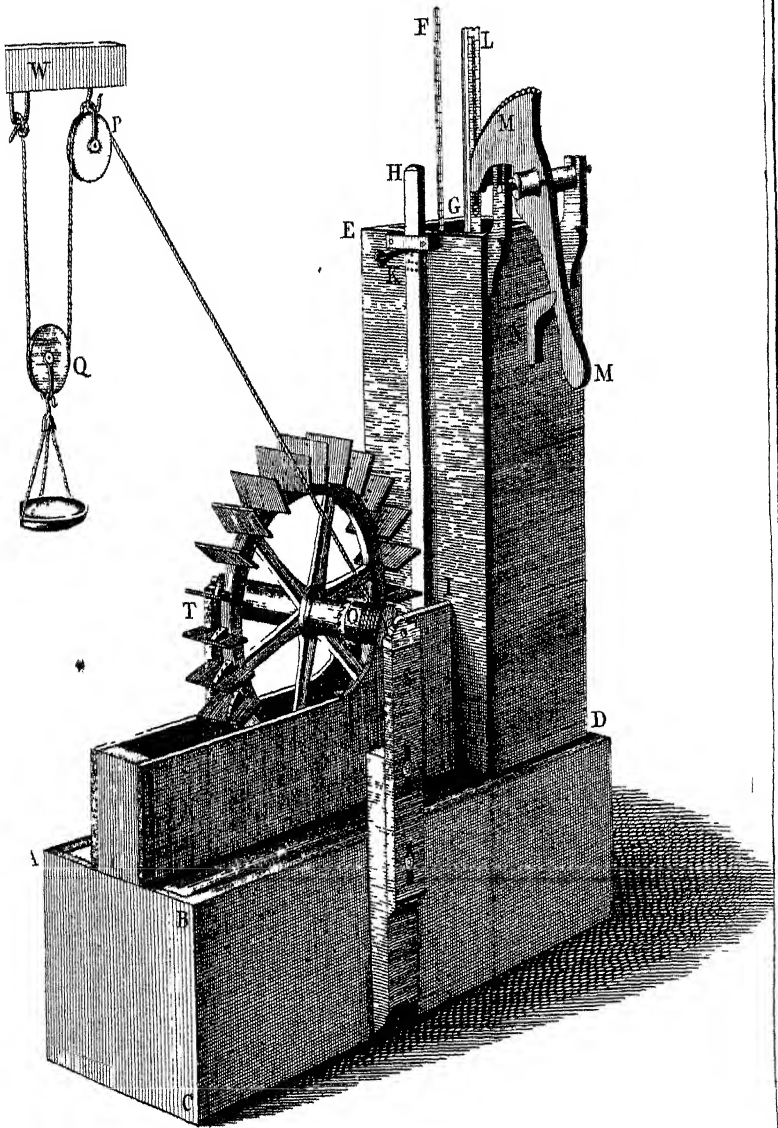
truth than towards it. Hence the common obferva-
tion, that a thing may do very well in a model, that
will not anfwer in large. And indeed, tho' the ut-
moft circumfpection be ufed in this way, the beft
ftructure of machines cannot be fully afcertained,
but by making trials with them, when made of their
proper fize. It is for this reafon, that, tho' the mo-
dels referred to, and the greateft part of the follow-
ing experiments, were made in the years 1752 and
1753, yet I deferred offering them to the Society,
till I had an opportunity of putting the deductions
made therefrom in real practice, in a variety of cafes,
and for various purpofes; fo as to be able to affure
the Society, that I have found them to anfwer.

PART I.

Concerning UNDERSHOT WATER-WHEELS.

PLATE IV. Fig. 1. is a perfpective view of the ma-
 chine for experiments on water-wheels; wherein

A B C D is the lower ciftern, or magazine, for re-
 ceiving the water, after it has quitted the wheel;
 and for fupplying

D E the upper ciftern, or head; wherein the water
 being raifed to any height required, by a pump,
 that height is fhewn by

F G, a fmall rod, divided into inches and parts;
 with a float at the bottom, to move the rod up
 and down, as the furface of the water rifes and
 falls.

H I is a rod by which the fluice is drawn, and ftopt
 at any height required, by means of

K a pin or peg, which fits feveral holes, placed

in the manner of a diagonal scale, upon the face
of the rod H I.

G L is the upper part of the rod of the pump, for
drawing the water out of the lower cistern, in
order to raise and keep up the surface thereof
at its desired height, in the head D E; thereby
to supply the water, expended by the aperture
of the sluice.

M M is the arch and handle for working the pump,
which is limited in its stroke by

N a piece for stopping the handle from raising the
piston too high; that also being prevented from
going too low, by meeting the bottom of the
barrel.

O is the cylinder, upon which a cord winds, and
which being conducted over the pullies P and
Q, raises

R, the scale, into which the weights are put, for
trying the power of the water.

S T the two standards, which support the wheel,
are made to slide up and down, in order to ad-
just the wheel, as near as possible, to the floor
of the conduit.

W the beam which supports the scale and pul-
lies; this is represented as but little higher than
the machine, for the sake of bringing the figure
into a moderate compass, but in reality is placed
15 or 16 feet higher than the wheel.

PLATE V. Fig. 2. is a section of the same machine,
wherein the same parts are marked with the same
letters as in Fig. 1. Besides which

X X is the pump barrel, being 5 inches diameter,
and 11 inches long.

Y is

Scale of Inches to fig 3.ᵈ

Fig 3.ᵈ

A   B   C   D   F

G

Fig 2.ᵈ

F

L

M

H

E

K

M

N

I

A

D

B   C
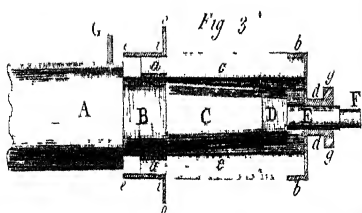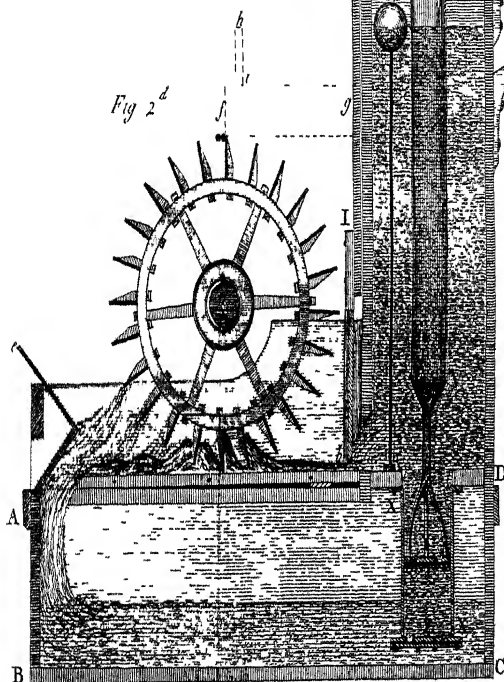
Scale of Feet to fig. 2.ᵈ

J Smeaton del.

J Mynde Sc.

Y is the pifton ; and

Z the fixed valve.

G V is a cylinder of wood, fixed upon the pump-rod, and reaches above the furface of the water ; this piece of wood being of fuch a thicknefs, that its fection is half the area of that of the pump-barrel, will caufe the furface of water to rife in the head, as much while the pifton is defcending, as while it is rifing : and will thereby keep the gauge-rod F G more equally to its height. *Note*, the arch and handle M M is here reprefented on a different fide to what it is fhewn in the preceding figures, in order that its dimenfions may the better appear.

a a fhews one of the two wires which ferve as directors to the float, in order that the gauge rod F G may be kept perpendicular ; for the fame purpofe alfo ferves *w*, a piece of wood with a hole to receive the gauge-rod, and keep it upright.

b is the aperture of the fluice.

c c a kant-board, for throwing the water more directly down the opening *c d*, into the lower ciftern : and

c e is a floping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3. reprefents one end of the main axis, with a fection of the moveable cylinder, marked O in the preceding figures.

A B C D is the end of the axis ; whereof the parts B and D are covered with ferrules or hoops of brafs.

E is a cylinder of metal ; whereof the part marked

F is

F is the pivot or gudgeon.

c c is the fection of an hollow cylinder of wood, the diameter of the interior part being some-what larger than the cylindrical ferrule B.

a a is the fection of a ferrule of brafs, driven into the end of the hollow cylinder, and which is adjufted to that marked B, fo as to flide freely thereupon, but with as little fhake as poffible.

b b, *d d*, *g g*, reprefent the fection of a brafs ferrule, plate, and focket, fixed upon the other end of the hollow cylinder; the focket *d d* being ad-jufted to flide freely upon the cylinder E, in the fame manner as the ferrule *a a* flides upon the cylinder B: the outer end of the focket at

g g is formed into a fort of button; by pufhing whereof, the hollow cylinder will move back-wards and forwards, or turn round at pleafure upon the cylindrical parts of the axis B and E.

e e, *i i*, *o o*, reprefent the fection of a brafs ferrule, al-fo fixed upon the hollow cylinder: the edge of this ferrule

e e is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

o o is cut in the manner of a ratchett.

Of confequence, when the plate *b d d b* is pufhed clofe to the ferrule D, the teeth of the ferrule *e e* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *g g*, the hollow cylinder is thereby dif-engaged from the pin G, and ceafes turning. *Note*. The weight in the fcale is prevented

from

from running back, by a catch that plays in and lays hold of the ratchet *o o.*

By this means the hollow cylinder upon which the cord winds, and raiſes the weight, is put in action and diſcharged therefrom inſtantaneouſly, while the wheel is in motion: for without ſome contrivance of this kind, it would not be eaſy to make this ſort of experiments with any tolerable degree of exactneſs.

The uſe of the apparatus now deſcribed will be rendered more intelligible, by giving a general idea of what I had in view; but as I ſhall be obliged to make uſe of a term which has heretofore been the cauſe of diſputation, I think it neceſſary to aſſign the ſenſe in which I would be uuderſtood to uſe it; and in which I apprehend it is uſed by practical *Mechanicks.*

The word *Power,* as uſed in practical mechanicks, I apprehend to ſignify the exertion of ſtrength, gravitation, impulſe, or preſſure, ſo as to produce motion: and by means of ſtrength, gravitation, impulſe, or preſſure, compounded with motion, to be capable of producing an effect: and that no effect is properly mechanical, but what requires ſuch a kind of power to produce it.

The raiſing of a weight, relative to the height to which it can be raiſed in a given time, is the moſt proper meaſure of power; or, in other words, if the weight raiſed is multiplied by the height to which it can be raiſed in a given time, the product is the meaſure of the power raiſing it; and conſequently, all thoſe powers are equal, whoſe products, made by

P ſuch

fuch multiplication, are equal: for if a power can raife twice the weight to the fame height; or the fame weight to twice the height, in the fame time that another power can, the firft power is double the fecond : and if a power can raife half the weight to double the height; or double the weight to half the height, in the fame time that another can, thofe two powers are equal. But note, all this is to be under-ftood in cafe of flow or equable motion of the body raifed; for in quick, accelerated, or retarded mo-tions, the *vis inertiæ* of the matter moved will make a variation.

In comparing the effects produced by water-wheels, with the powers producing them; or, in other words, to know what part of the original power is neceffarily loft in the application, we muft previoufly know how much of the power is fpent in overcoming the friction of the machinery, and the refiftance of the air; alfo what is the real velo-city of the water at the inftant that it ftrikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the inftant that it ftrikes the wheel, given; the height of head pro-ductive of fuch velocity can be deduced, from ac-knowleged and experimented principles of hydrofta-tics : fo that by multiplying the quantity, or weight of water, really expended in a given time, by the height of head fo obtained; which muft be confi-dered as the height from which that weight of wa-ter had defcended in that given time ; we fhall have a product, equal to the original power of the water; and clear of all uncertainty, that would arife from the friction of the water, in paffing fmall apertures;

and

and from all doubts, arifing from the different mea-
fure of fpouting waters, affigned by different authors.
On the other hand, the fum of the weights raifed
by the action of this water, and of the weight re-
quired to overcome the friction and refiftance of the
machine, multiplied by the height to which the
weight can be raifed in the time given, the product
will be equal to the effect of that power; and the
proportion of the two products will be the propor-
tion of the *power* to the *effect* : fo that by loading
the wheel with different weights fucceffively, we
fhall be able to determine at what particular load,
and velocity of the wheel, the effect is a *maximum*.

The manner of finding the real velocity of the
water, at the inftant of its ftriking the wheel; the
manner of finding the value of the friction, refift-
ance, &c. in any given cafe; and the manner of
finding the real expence of water, fo far as con-
cerns the following experiments, without having re-
courfe to theory; being matters upon which the fol-
lowing determinations depend, it will be neceffary
to explain them.

To determine the Velocity of the Water ftriking the
Wheel.

It has already been mentioned, in the references
to the figures, that weights are raifed by a cord
winding round a cylindrical part of the axis. Firft,
then, let the wheel be put in motion by the water,
but without any weights in the fcale; and let the
number of turns in a minute be 60: now it is evi-
dent, that was the wheel free from friction and refift-
ance, that 60 times the circumference of the wheel

would

would be the fpace through which the water would
have moved in a minute; with that velocity where-
with it ftruck the wheel: but the wheel being in-
cumbred by friction and refiftance, and yet moving
60 turns in a minute, it is plain, that the velocity
of the water muft have been greater than 60 cir-
cumferences before it met with the wheel. Let
now the cord be wound round the cylinder, but
contrary to the ufual way, and put a weight in
the fcale; the weight fo difpofed (which may ·be
called the *counter-weight)* will endeavour to affift the
wheel in turning the fame away, as it would have
been turned by the water: put therefore as much
weight into the fcale as, without any water, will
caufe it to turn fomewhat fafter than at the rate of
60 turns in a minute; fuppofe 63: let it now be
tried again by the water, affifted by the weight; the
wheel therefore will now make more than 60 turns;
fuppofe 64: hence we conclude the water ftill ex-
erts fome power in giving motion to the wheel. Let
the weight be again increafed, fo as to make $64\frac{1}{2}$
turns in a minute without water: let it once more
be tried with water as before; and fuppofe it now
to make the fame number of turns with water as
without. *viz.* $64\frac{1}{2}$: hence it is evident, that in this
cafe the wheel makes the fame number of turns in
a minute, as it would do if the wheel had no fric-
tion or refiftance at all; becaufe the weight is equi-
valent thereto; for was it too little, the water would
accelerate the wheel beyond the weight; and if too
great, retard it; fo that the water now becomes a
regulator of the wheel's motion; and the velocity
of its circumference becomes a meafure of the velo-
city of the water.

In

In like manner, in feeking the greateſt product, or *maximum* of effect; having found by trials what weight gives the greateſt product, by ſimply multi- plying the weight in the ſcale by the number of turns of the wheel, find what weight in the ſcale, when the cord is on the contrary ſide of the cylinder, will cauſe the wheel to make the ſame number of turns the ſame way, without water; it is evident that this weight will be nearly equal to all friction and reſiſt- ance taken together; and conſequently, that the weight *in* the ſcale, with twice * the weight *of* the ſcale, added to the back or counter-weight, will be equal to the weight that could have been raiſed, ſup- poſing the machine had been without friction or re- ſiſtance; and which multiplied by the height to which it was raiſed, the product will be the greateſt effect of that power.

The quantity of water expended is found thus:

The pump made uſe of for repleniſhing the head with water was ſo carefully made, that no water eſcaping back by the leathers, it delivered the ſame quantity of water at every ſtroke, whether worked quick or ſlow; and as the length of the ſtroke was limited, conſequently the value of one ſtroke (or on account of more exactneſs 12 ſtrokes) was known, by the height to which the water was thereby raiſed in the head; which being of a regular figure was eaſily meaſured. The ſluice, by which the water was drawn upon the wheel, was made to ſtop at certain heights by a peg; ſo that when the peg was in the ſame hole,

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* The weight of the ſcale makes part of the weight both ways.

the aperture for the effluent water was the fame. Hence the quantity of water expended by any given head, and opening of the fluice, may be obtained : for by obferving how many ftrokes a minute was fuffi-cient to keep up the furface of the water at the given height, and multiplying the number of ftrokes by the value of each, the water expended by any given aperture and head in a given time will be given.

Thefe things will be further illuftrated by going over the *calculus* of one fett of experiments.

Specimen of a Sett of Experiments.

The fluice drawn to the 1ft hole.

The water above the floor of the fluice   30 Inches.
Strokes of the pump in a minute   — $39\frac{1}{2}$
The head raifed by 12 ftrokes — 21 Inches.
The wheel raifed the empty fcale, and made turns
   in a minute — —   80
With a counter-weight of 1 lb. 8 oz. it made   85
D° tried with water — — — 86

N°		Weight.		Turns in a min.		Product.
		lb.	oz.			
1	—	4	0	—	45	—	180
2	—	5	0	—	42	—	210
3	—	6	0	—	$36\frac{1}{4}$	—	$217\frac{1}{2}$
4	—	7	0	—	$33\frac{3}{4}$	—	$236\frac{1}{4}$
5	—	8	0	—	30	—	240 maximum.
6	—	9	0	—	$26\frac{1}{2}$	—	$238\frac{1}{4}$
7	—	10	0	—	22	—	220
8	—	11	0	—	$16\frac{1}{4}$	—	$181\frac{1}{2}$
9	—	12	*	ceafed working.			

* *N. B.* When the wheel moves fo flow as not to rid the wa-ter fo faft as fupplied by the fluice, the accumulated water falls back upon the aperture, and the wheel immediately ceafes moving.

Counter

Counter-weight, for 30 turns without water, 2 oz. in the fcale.

N. B. The area of the head was 105,8 fquare inches.

Weight of the empty fcale and pulley, 10 oz.
Circumference of the cylinder, 9 inches.
Circumference of the water-wheel, 75 ditto.

Reduction of the above Sett of Experiments.

The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a fecond, equal to 107,5 inches, or 8,96 feet, which is due to a head of 15 inches *; and this we call the *virtual* or *effective* head.

The area of the head being 105,8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ,579 of the ounce avoirdupoife, gives 61,26 ounces for the weight of as much water, as is contained in the head, upon 1 inch in depth, $\frac{1}{16}$ of which is 3,83 pounds; this multiplied by the depth 21 inches, gives 80,43 lb. for the value of 12 ftrokes; and by proportion, $39\frac{1}{2}$ (the number made in a minute) will give 264,7 lb. the weight of water expended in a minute.

Now as 264,7 lb. of water may be confidered as having defcended through a fpace of 15 inches in a minute, the product of thefe two numbers 3970 will exprefs the *power* of the water to produce mechanical effects; which were as follows.

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* This is determined upon the common maxim of hydroftatics, that the velocity of fpouting waters is equal to the velocity that an heavy body would acquire in falling from the height of the refer/or; and is proved by the rifing of jets to the height of their refervoirs nearly.

The velocity of the wheel at the *maximum*, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the fcale was hung by a pulley and double line, the weight was only raifed half of this, *viz.* 135 inches.

The weight in the fcale at the maximum   8 *lb.* 0 *oz.*
Weight of the fcale and pulley   ——   0   10
Counterweight, fcale, and pulley   ——   0   12

Sum of the refiftance   —— ——   9   6

or *lb.* 9,375.

Now as 9,375 lb. is raifed 135 inches, thefe two numbers being multiplied together, the product is 1266, which expreffes the effect produced at a maximum : fo that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3,18.

But tho' this is the greateft *fingle* effect producible from the power mentioned, by the impulfe of the water upon an underfhot wheel; yet, as the whole power of the water is not exhaufted thereby, this will not be the true ratio between the *power* of the water, and the *fum* of all the *effects* producible therefrom : for as the water muft neceffarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that fome part of the power of the water muft remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns a minute; and confequently its circumference moves at the rate of 3,123 feet a fecond, which anfwers to a head 1,82 inches; this being multiplied by the expence of water in a minute, *viz.* 264,7 lb. produces 481 for the power *remaining* in the water after it has paffed the wheel: this being therefore deducted

leducted from the original power 3970, leaves 3489, which is that *part* of the power which is spent in producing the effect 1266; and consequently the part of the power spent in producing the effect, is to the greatest effect producible thereby as 3489 : 1266 : 10 : 3,62,or as 11 to 4.

The *velocity of the water* striking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the *velocity of the wheel* at the *maximum* to be 30 ; the velocity of the water will therefore be to that of the wheel as 86 to 30, or as 10 to 3,5, or as 20 to 7.

The *load at the maximum* has been shown to be equal to 9 lb. 6 oz. and that the wheel ceased moving with 12 lb. in the scale : to which if the weight of the scale is added, *viz.* 10 ounces *, the proportion will be nearly as 3 to 4 between the load at the *maximum* and *that* by which the wheel is stopped.

It is somewhat remarkable, that tho' the velocity of the wheel in relation to the water turns out greater than $\frac{1}{3}$ of the velocity of the the water, yet the impulse of the water in the case of a *maximum* is more than double of what is assigned by theory ; that is, instead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It must be remembred, therefore, that, in the present case, the wheel was not placed in an open river, where the natural current, after it has communicated its impulse to the float, has room on all sides to escape, as the theory supposes ; but in a conduit or

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* The resistance of the air in this case ceases, and the friction is not added, as 12 lb. in the scale was sufficient to stop the wheel after it had been in full motion ; and therefore somewhat more than a counterbalance to the impulse of the water.

race, to which the float being adapted, the water cannot otherwise escape than by moving along with the wheel. It is observable, that a wheel working in this manner, as soon as the water meets the float, receiving a sudden check, it rises up against the float, like a wave against a fixed object; infomuch that when the sheet of water is not a quarter of an inch thick before it meets the float, yet this sheet will act upon the whole surface of a float, whose height is 3 inches; and consequently was the float no higher than the thickness of the sheet of water, as the theory also supposes, a great part of the force would have been lost, by the water's dashing over the float *.

In further confirmation of what is already delivered, I have adjoined the following table, containing the result of 27 setts of experiments, made and reduced in the manner above specified. What remains of the theory of undershot wheels, will naturally follow from a comparison of the different experiments together.

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* Since the above was wrote, I find that Professor Euler, in the Berlin Acts for the year 1748, in a memoire intitled, *Maxims pour aranger le plus avantageusment les machines destinées à elever de l'eau par le moyen de pompes*, page 192. § 9. has the following passage; which seems to be the more remarkable, as I don't find he has given any demonstration of the principle therein contained, either from theory or experiment; or has made any use thereof in his calculations on this subject.——" Cependant dans ce cas puisque " l'eau est reflechie, & qu'elle decoule sur les aubes vers les cotes, " elle y exerce encore une force particuliere, dont l'effet de l'im- " pulsion sera augmenté; & experience jointe a la theorie a fait " voir que dans ce cas, la force est presque double : de sorte " qu'il faut prendre le double de le section du fil d'eau pour ce " qui repond dans ce cas a le surface des aubes, pourvu qu'elles " soient assez larges pour recevoir ce supplement de force. Car si " les aubes nétoient plus larges que le fil, on trait d'eau on ne " devroit prendre que ne simple section, tout comme dans le pre- " mier cas, ou l'aube toute entiere est pappee par l'eau."

TABLE

TABLE I.

N°	Height of the water in the cistern.	Turns of the wheel un-loaded.	Virtual head deduced therefrom.	Turns at the maximum.	Load at the equilibrium.	Load at the maximum.	Water expended in a minute.	Power.	Effect.	Ratio of the power and effect.	Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibrium, to the load at the maximum.	Experiments.
	In.		In.		lb. oz.	lb. oz.							
1	33	88	15,85	30,	13 10	10 9	275,	4358	1411	10:3,24	10:3,4	10:7,75	
2	30	86	15,0	30,	12 10	9 6	264,7	3970	1266	10:3,2	10:3,5	10.7,4	
3	27	82	13,7	28,	11 2	8 6	243,	3329	1044	10:3,15	10:3,4	10:7,5	
4	24	78	12,3	27,7	9 10	7 5	235,	2890	901,4	10:3,12	10:3,55	10:7,53	At
5	21	75	11,4	25,9	8 10	6 5	214,	2439	735,7	10:3,02	10:3,45	10:7,32	the
6	18	70	9,95	23,5	6 10	5 5	199,	1970	561,8	10:2,85	10:3,36	10:8,02	1st
7	15	65	8,54	23,4	5 2	4 4	178,5	1524	442,5	10:2,9	10:3,6	10:8,3	hole.
8	12	60	7,29	22,	3 10	3 5	161,	1173	328	10:2,8	10:3,77	10:9,1	
9	9	52	5,47	19,	2 12	2 8	134,	733	213,7	10:2,9	10:3,65	10:9,1	
10	6	42	3,55	16,	1 12	1 10	114,	404,7	117	10:2,82	10:3,8	10:9,3	
11	24	84	14,2	30,75	13 10	10 14	342,	4890	1505	10:3,075	10:3,66	10:7,9	
12	21	81	13,5	29,	11 10	9 6	297,	4009	1223	10:3,01	10:3,62	10:8,05	
13	18	72	10,5	26,	9 10	8 7	285,	2993	975	10:3,25	10:3,6	10:8,75	At
14	15	69	9,6	25,	7 10	6 14	277,	2659	774	10:2,92	10:3,62	10:9,	the
15	12	63	8,0	25,	5 10	4 14	234,	1872	549	10:2,94	10:3,97	10:8,7	2d.
16	9	56	6,37	23,	4 0	3 13	201,	1280	390	10:3,05	10:4,1	10:9,5	
17	6	46	4,25	21,	2 8	2 4	167,5	712	212	10:2,98	10:4,55	10:9,	
18	15	72	10,5	29,	11 10	9 6	357,	3748	1210	10:3,23	10:4,02	10:8,05	
19	12	66	8,75	26,75	8 10	7 6	330,	2887	878	10:3,05	10:4,05	10:8,1	The
20	9	58	6,8	24,5	5 8	5 0	255,	1734	541	10:3,01	10:4,22	10:9,1	3d.
21	6	48	4,7	23,5	3 2	3 0	228,	1064	317	10:2,99	10:4,9	10:9,6	
22	12	68	9,3	27,	9 2	8 6	359,	3338	1006	10:3,02	10:3,97	10:9,17	
23	9	58	6,8	26,25	6 2	5 13	332,	2257	686	10:3,04	10:4,52	10:9,5	4th.
24	6	48	4,7	24,5	3 12	3 8	262,	1231	385	10:3,13	10:5,1	10:9,35	
25	9	60	7,29	27,3	6 12	6 6	355,	2588	783	10:3,03	10:4,55	10:9,45	5th.
26	6	50	5,03	24,6	4 6	4 1	307,	1544	450	10:2,92	10:4,9	10:9,3	
27	6	50	5,03	26,	4 15	4 9	360,	1811	534	10:2,95	10:5,2	10:9,25	6th.
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11,	12.	13.

Maxims

Maxims and Observations deduced from the foregoing Table of Experiments.

Maxim I. *That the virtual or effective head being the same, the effect will be nearly as the quantity of water expended.*

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing setts of experiments; as for

Example 1*st, taken from* N°. 8. *and* 25, viz.

No.	Virtual Head.	Water expended.	Effect.
8	7,29	161	328
25	7,29	355	785

Now the heads being equal, if the effects are proportioned to the water expended, we shall have by maxim 1st, 161 : 355 :: 328 : 723; but 723 falls short of 785, as it turns out in experiment according to N°. 25, by 62; the effect therefore of N°. 25, compared with N°. 8, is greater than according to the present maxim in the ratio of 14 to 13.

The foregoing example, with four similar ones, are seen at one view in the following Table.

Examples	N° Tab. I.	Virtual Head.	Expence of Water.	Effect.	Comparison.	Variation	Proportional Variation.
		Inch.	*lb.*				
1st {	8	7,29	161	328	{ 161 : 355 :: 328 : 723	62+	14 : 13
	25	7,29	355	785			
2d {	13	10,5	285	975	{ 285 : 357 :: 975 : 1221	11—	121:122
	18	10,5	357	1210			
3d {	22	6,8	255	541	{ 255 : 332 :: 541 : 704	18—	38 : 39
	23	6,8	332	686			
4th {	21	4,7	228	317	{ 228 : 262 :: 317 : 364	21+	18 : 17
	24	4,7	262	385			
5th {	26	5,03	307	450	{ 307 : 360 :: 450 : 531	3 +	178:177
	27	5,03	360	534			

Hence

Hence therefore, in comparing different experiments, as some fall short, and others exceed the maximum, and all agree therewith, as near as can be expected, in an affair where so many different circumstances are concerned; we may, according to the laws of reasoning by induction, conclude the maxim true; *viz.* that the effects are nearly as the quantity of water expended.

Maxim II. *That the expence of water being the same, the effect will be nearly as the height of the virtual or effective head.*

This also will appear by comparing the contents of columns 4, 8, and 10, in any of the setts of experiments.

Example 1*st*, *of* N°. 2. *and* N°. 24. *viz.*

N°.	Virt. Head.	Expence.	Effect.
2 ——	15 ——	264,7 ——	1266
24 ——	4,7 ——	262 ——	385

Now as the expences are not quite equal, we must proportion one of the effects accordingly: thus

by maxim 1st, 262 : 264,7 :: 385 : 389
and by max. 2d, 15 : 4,7 :: 1266 : 397

Difference —— 8

The effect therefore of N°. 24. compared with N°. 2. is less than according to the present maxim in the ratio of 49 : 50.

The foregoing, and two other similar examples, are comprised in the following Table.

Examples

Examples.	N°. Tab.I.	Virtual Head.	Expence of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1ft	2 24	15 4,7	264,7 262	1266 385	Max. 1ft, 262 : 264,7 :: 385 : 319 Max. 2d, 15 : 4,7 :: 1266 : 397	8 —	49 : 50
2d	1 10	15,85 3,55	275 114	1411 117	Max. 1ft, 114 : 275 :: 117 : 282 Max. 2d. 15,85 : 3,55 :: 1411 : 316	34—	8 : 9
3d	11 17	14,2 4,25	342 1675	1505 212	Max. 1ft, 167,5 : 342 :: 212 : 433 Max. 2d, 14,2 : 4,25 :: 1505 : 450	17—	25 : 26

Maxim III. *That the quantity of water expended being the same, the effect is nearly as the square of its velocity.*

This will appear by comparing the contents of columns 3, 8, and 10, in any of the fetts of experiments; as for

Example 1ft *of* N°. 2. *with* N°. 24. *viz.*

N°.	Turns in a min.	Expence.	Effect.
2	86	264,7	1266
24	48	262	385

The velocity being as the number of turns, we fhall have,

$$\text{by max. 1ft,} \quad 262 : 264,7 :: 385 : 389$$

$$\text{and by max. 3d,} \left\{ \begin{array}{l} 86^2 : 48^2 \\ 7396 : 2304 \end{array} \right\} :: 1266 : 394$$

$$\text{Difference} \text{ - - - } \quad 5$$

The effect therefore of N°. 24. compared with N°. 2. is lefs than by the prefent maxim in the ratio of 78 : 79.

The foregoing, and three other fimilar examples, are comprifed in the following Table.

Examples,

Examples.	No. Tab. I.	Turns in a minute.	Expence of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1st	2	86	264,7	1266	Max. 1st, 262 : 264,7 :: 385 : 389	{5	78 : 79
	24	48	262	385	Max. 3d, 86^2 : 48^2 :: 1266 : 394 / 7396 : 2304		
2d	1	88	275	1411	Max. 1st, 114 : 275 :: 117 : 282	{39	7 : 8
	10	42	114	117	Max. 3d, 88^2 : 42^2 :: 1411 : 321 / 7744 : 1764		
3d	11	84	342	1505	Max. 1st, 167,5 : 342 :: 212 : 433	{18	24 : 25
	17	46	167,5	212	Max. 3d, 84^2 : 46^2 :: 1505 : 451 / 7056 : 2116		
4th	18	72	357	1210	Max. 1st, 228 : 357 :: 317 : 496	{42	12 : 13
	21	48	228	317	Max. 3d, 72^2 : 48^2 :: 1210 : 538 / 5184 : 2301		

Maxim

Maxim 4th. *The aperture being the same, the effect will be nearly as the cube of the velocity of the water.*

This also will appear by comparing the contents of columns 3, 8, and 10; as for

Example 1*st*, *of* N°. 1. *and* N°. 10, *viz.*

N°.	Turns.	Expence.	Effect.
1	—— 88	—— 275	—— 1411
10	—— 42	—— 114	117

Lemma. It must here be observed, that if water passes out of an aperture, in the same section, but with different velocities; the expence will be proportional to the velocity; and therefore conversely, if the expence is not proportional to the velocity, the section of the water is not the same.

Now comparing the water discharged with the turns of N°. 1. and 10, we shall have 88 : 42 :: 275 : 131,2; but the water discharged by N°. 10. is only 114 lb. therefore, tho' the sluice was drawn to the same height in N°. 10. as in N°. 1. yet the section of the water passing out, was less in N°. 10. than N°. 1. in the proportion of 114 to 131,2; consequently had the effective aperture or section of the water been the same in N°. 10. as in N°. 1. so that 131,2 lb. of water had been discharged instead of 114, the effect would have been increased in the same proportion; that is,

by the *Lemma,*   88 : 42 :: 275 : 131,2

by maxim 1st,   114 : 131,2 :: 117 : 134,5

and by max. 4th, $\left\{\begin{matrix} 83^3 : & 42^3 \\ 681472 : & 74088 \end{matrix}\right\}$ :: 1411 : 153,5

Difference —— 19

The

The effect therefore of N°. 10. compared with N°. 1. is less than it ought to be by the present maxim in the ratio of 7 : 8.

The foregoing, and three other similar examples, are contained in the following Table.

Examples.	N°.Tab.I.	Turns in a minute.	Expence of water.	Effect.	Comparison.	Variation.	Proportional Variation.
1ft	1, 10	88, 42	275, 114	1411, 117	Lemma. 88 : 42 :: 275 : 131,2 } 131,2 Max.1. 114 : 131,2 :: 117 : 134,5 } 134,5 Max.4. 88³ : 42³ :: 1411 : 153,5 } 153,5	19—	7 : 8
2d	11, 17	84, 46	342, 167,5	1505, 212	Lemma. 84 : 46 :: 342 : 187,3 } 187,3 Max.1. 167,5 : 187,3 :: 212 : 237 } 237 Max.4. 84³ : 46³ :: 1505 : 247 } 247	10—	23 : 24
3d	18, 21	72, 48	357, 228	1210, 317	Lemma. 72 : 48 :: 357 : 238 } 238 Max.1. 228 : 238 :: 317 : 331 } 331 Max.4. 72³ : 48³ :: 1210 : 355 } 355	24—	14 : 15
4t	22, 24	68, 48	359, 262	1006, 385	Lemma. 68 : 48 :: 359 : 253,4 } 253,4 Max.1. 262 : 253,4 :: 385 : 372 } 372 Max.4. 68³ : 48³ :: 1006 : 354 } 354	18+	20 : 19

OBSERVATIONS.

Obſerv. 1ſt. On comparing column 2d and 4th, Tab. I. it is evident, that the *virtual head* bears no certain proportion to the *head of water* ; but that when the aperture is greater, or the velocity of the water iſſuing therefrom leſs, they approach nearer to a coincidence : and conſequently in the large openings of mills and ſluices, where great quantities of water are diſcharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

Obſerv. 2d. Upon comparing the ſeveral proportions between the *power* and *effect* in column 11th, the moſt general is that of 10 to 3 ; the extremes 10 to 3,2 and 10 to 2,8 ; but as it is obſervable, that where the quantity of water, or the velocity thereof; that is, where the power is greateſt, the 2d term of the ratio is greateſt alſo : we may therefore well allow the proportion ſubſiſting in large works, as 3 to 1.

Obſerv. 3d. The proportions of *velocities* between the *water* and *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1 ; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approach to that of 2 to 1, the beſt general proportion will be that of 5 to 2.

Obſerv. 4th. On comparing the numbers in column 13, it appears, that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally ſtop it ; but that they are contained within the limits of 20 to 19, and

of

of 20 to 15; but as the effect approaches neareſt to the ratio of 20 to 15, or of 4 to 3, when the power is greateſt, whether by increaſe of velocity, or quantity of water, this ſeems to be the moſt applicable to large works: but as the load that a wheel ought to have, in order to work to the beſt advantage, can be aſſigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it; the exact knowlege of the greateſt load it will bear, is of the leſs conſequence in practice.

It is to be noted, that in all the examples under the three laſt of the four preceding maxims, the effect of the leſſer power falls ſhort of its due proportion to the greater, when compared by its maxim; except the laſt example of maxim 4th: and hence, if the experiments are taken ſtrictly, we muſt infer, that the effects increaſe and diminiſh in an higher ratio than thoſe maxims ſuppoſe: but as the deviation is not very conſiderable, the greateſt being about 1-8th of the quantity in queſtion; and as it is not eaſy to make experiments of ſo compounded a nature with abſolute preciſion; we may rather ſuppoſe, that the leſſer power is attended with ſome friction, or works under ſome diſadvantage, which has not been duly accounted for, and therefore we may conclude, that theſe maxims will hold very nearly, when applied to works in large.

After the experiments above mentioned were tried, the wheel, which had originally 24 floats, was reduced to twelve; which cauſed a diminution in the effect, on account of a greater quantity of water eſcaping between the floats and the floor; but a cir-

cular

cular fweep being adapted thereto, of fuch a length,
that one float entered the curve before the preceding
one quitted it, the effect came fo near to the former,
as not to give hopes of advancing it by increafing
the number of floats beyond 24 in this particular
wheel.

PART II.

Concerning OVERSHOT WHEELS.

Read May 24, IN the former part of this effay, we have
1759.   confidered the impulfe of a confined
ftream, acting on *Underfhot Wheels*. We now pro-
ceed to examine the power and application of water,
when acting by its *gravity* on *Overfhot Wheels*.

In reafoning without experiment, one might be
led to imagine, that however different the mode of
application is; yet that whenever the fame quantity
of water defcends thro' the fame perpendicular fpace,
that the natural effective power would be equal;
fuppofing the machinery free from friction, equally
calculated to receive the full effect of the power, and
to make the moft of it : for if we fuppofe the height
of a column of water to be 30 inches, and refting
upon a bafe or aperture of one inch fquare; every
cubic inch of water that departs therefrom will ac-
quire the fame velocity or *momentum*, from the
uniform preffure of 30 cubic inches above it, that
one cubic inch let fall from the top will acquire in
falling down to the level of the aperture; *viz.* fuch
a velocity as in a contrary direction would carry it to
the

the level from whence it fell ; * one would therefore
fuppofe, that a cubic inch of water, let fall thro' a
fpace of 30 inches, and there impinging upon an-
other body, would be capable of producing an equal
effect by collifion, as if the fame cubic inch had de-
fcended thro' the fame fpace with a flower motion,
and produced its effects gradually: for in both cafes
gravity acts upon an equal quantity of matter, thro'
an equal fpace † ; and confequently, that whatever
was the ratio between the power and effect in under-
fhot wheels, the fame would obtain in overfhot, and
indeed in all others : yet, however conclufive this
reafoning may feem, it will appear, in the courfe of
the following deductions, that the effect of the gra-
vity of defcending bodies is very different from the
effect of the ftroke of fuch as are *non-elaftic*, tho'
generated by an equal mechanical power.

The alterations in the machinery already defcribed,
to accommodate the fame for experiments on over-
fhot wheels, were principally as follows.

P L A T E V. *Fig.* 2. The fluice I *b* being fhut
down, the rod H I was unfcrewed and taken off.

The underfhot water-wheel was taken off the axis,
and inftead thereof an overfhot wheel of the fame

---

* This is a confequence of the rifing of jetts to the height of
their refervoirs nearly.

† Gravity, it is true, acts a longer fpace of time upon the body
that defcends flow than upon that which falls quick ; but this can-
not occafion the difference in the effect: for an elaftic body falling
thro' the fame fpace in the fame time, will, by collifion upon an-
other elaftic body, rebound nearly to the height from which it fell ;
or, by communicating its motion, caufe an equal one to afcend
to the fame height.

<div align="right">diameter</div>

diameter was put into its place. *Note*, This wheel was two inches in the fhroud or depth of the bucket; the number of the buckets was 36.

The ftandards S and T, *Fig.* 1. were raifed half an inch, fo that the bottom of the wheel might be clear of ftagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, *Fig.* 2. The aperture was adjufted by a fhuttle *h i*, which alfo clofed up the outer end of the trunk, when the water was to be ftopped.

Fig. 3. The ratchet *o o*, not being of one piece of metal with the ferrule *e e*, *i i* (tho' fo defcribed before, to prevent unneceffary diftinctions), was with its catch turned the contrary fide; confequently the moveable barrel would do its office equally, notwith-ftanding the water-wheel, when at work, moved the contrary way.

Specimen of a Sett of Experiments.

Head 6 inches.

14 $\frac{1}{2}$ ftrokes of the pump in a minute, 12 ditto = 80 lb. *

Weight of the fcale (being wet) 10 $\frac{1}{2}$ oz.

Counterweight for 20 turns, befides the fcale, 3 oz.

No.	Weight in the Scale.	Turns.	Product.	Obfervations.
1	0 *lb.*	60		Threw moft part
2	1	56		of the water out
3	2	52		of the wheel.
4	3	49	147	Received the water more quietly.
5	4	47	188	
6	5	45	225	
7	6	42 $\frac{1}{2}$	255	
8	7	41	287	
9	8	38 $\frac{1}{2}$	308	
10	9	36 $\frac{1}{2}$	328 $\frac{1}{2}$	
11	10	35 $\frac{1}{2}$	355	
12	11	32 $\frac{3}{4}$	360 $\frac{1}{4}$	
13	12	31 $\frac{1}{4}$	375	
14	13	28 $\frac{1}{2}$	370 $\frac{1}{2}$	
15	14	27 $\frac{1}{2}$	385	
16	15	26	390	
17	16	24 $\frac{1}{2}$	392	
18	17	22 $\frac{3}{4}$	386 $\frac{3}{4}$	
19	18	21 $\frac{3}{4}$	391 $\frac{1}{2}$	
20	19	20 $\frac{3}{4}$	394 $\frac{1}{4}$	Maximum.
21	20	19 $\frac{3}{4}$	395	
22	21	18 $\frac{1}{4}$	388 $\frac{1}{4}$	
23	22	18	396	Work'd irregular.
24	23	Overfet by its load.		

* The fmall difference, in the value of 12 ftrokes of the pump, from the former experiments, was owing to a fmall difference in the length of the ftroke, occafioned by the warping of the wood.

Reduction

Reduction of the preceding Specimen.

In thefe experiments the head being 6 inches, and the height of the wheel 24 inches, the whole defcent will be 30 inches: the expence of water was 14 ½ ftrokes of the pump in a minute, whereof 12 contained 80 lb.; therefore the water expended in a minute was 96 ⅔ lb. which, multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we fhall have 20 ¾ turns in a minute, each of which raifed the weight 4 ½ inches, that is, 93,37 inches in a minute. The weight *in* the fcale was 19 lb, the weight *of* the fcale 10½ oz.; the counter-weight 3 oz. in the fcale, which, with the weight of the fcale 10½ oz. makes in the whole 20½ lb. which is the whole refiftance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The *ratio* therefore of the *power* and *effect* will be as 2900 : 1914, or as 10 : 6,6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we fhall have 96 ⅔ lb. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 82, or as 5 : 4 nearly.

The reduction of this fpecimen is fet down in N°. 9. of the following Table; and the reft were deducted from a fimilar fett of experiments, reduced in the fame manner.

TABLE II. *containing the Result of Sixteen Setts of Experiments on Overshot Wheels.*

N°.	Whole descent.	Water expended in a minute.	Turns at the maximum in a min.	Weight raised at the maximum.	Power of the whole descent.	Power of the wheel.	Effect.	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean ratio.
	Inch	*lb.*		*lb.*						
1	27	30	19	6½	810	720	556	10:6,9	10:7,7	Medium 10: 8,1
2	27	56⅔	16¼	14½	1530	1360	1060	10:6,9	10:7,8	
3	27	56⅔	20¾	12½	1530	1360	1167	10:7,6	0:8,4	
4	27	63½	20½	13½	1710	1524	1245	10:7,3	10:8,2	
5	27	76⅔	21½	15½	2070	1840	1500	10:7,3	10.8,2	
6	28½	73⅓	18¾	17½	2090	1764	1476	10:7,	10:8,4	10:8,2
7	28¼	96⅔	20¼	20½	2755	2320	1868	10:6,8	10:8,1	
8	30	90	20	19½	2700	2160	1755	10:6,5	10:8,1	10:8,2
9	30	96⅔	20¾	20½	2900	2320	1917	10:6,6	10.8,2	
10	30	113½	21	23½	3400	2720	2221	10:6,5	10:8,2	
11	33	56⅔	20¼	13½	1870	1360	1230	10:6,6	10:9,	10:8,5
12	33	106⅔	22¼	21½	3520	2560	2153	10:6,1	10.8,4	
13	33	146⅔	23	27½	4840	3520	2846	10:5,9	10:8,1	
14	35	65	19¾	16½	2275	1560	1466	10:6,5	10:9,4	10:8,5
15	35	120	21½	25½	4200	2880	2467	10:5,9	10.8,6	
16	35	163½	25	26½	5728	3924	2981	10.5,2	10:7,6	
1	2.	3	4	5.	6.	7.	8.	9.	10	11

Observations and Deductions from the foregoing Experiments.

I. *Concerning the Ratio between the Power and Effect of Overshot Wheels.*

The effective power of the water must be reckoned upon the whole descent; because it must be

raised

raifed that height, in order to be in a condition of producing the fame effect a fecond time.

The ratio's between the *powers* fo eftimated, and the *effects* at the *maximum* deduced from the feveral fetts of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that thofe ratio's differ from that of 10 to 7,6 to that of 10 : 5,2, that is, nearly from 4 : 3 to 4 : 2. In thofe experiments where the heads of water and quantities expended are leaft, the proportion is nearly as 4 : 3 ; but where the heads and quantities are greateft, it approaches nearer to that of 4 : 2 ; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have feen before, in our obfervations upon the effects of underfhot wheels, that the general ratio of the power to the effect, when greateft, was 3 : 1 ; *the effect therefore of overfhot wheels, under the fame circumftances of quantity and fall, is at a medium double to that of the underfhot :* and, as a confequence thereof, *that nonelaftic bodies, when acting by their impulfe or collifion, communicate only a part of their original power ;* the other part being fpent in changing their figure in confequence of the ftroke.

The powers of water computed from the height of the wheel only, compared with the effects, as in column 10. appear to obferve a more conftant ratio : for if we take the medium of each clafs, which is fet down in column 11, we fhall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10 : 8,5 ; and as the fecond term of the ratio gradually increafes from 8,1 to 8,5, by an increafe of head from 3 inches to 11, the excefs of 8,5 above

8,1.

8,1 is to be imputed to the superior impulse of the water at the head of 11 inches above that of 3 inches: so that if we reduce 8,1 to 8, on account of the impulse of the 3 inch head, *we shall have the ratio of the power, computed upon the height of the wheel only, to the effect at a maximum as* 10 : 8, *or as 5 : 4 nearly :* and from the equality of the ratio between power and effect, subsisting where the constructions are similar, we must infer, *that the effects,* as well as the powers, *are as the quantities of water and perpendicular heights multiplied together respectively.*

II. *Concerning the most proper Height of the Wheel in proportion to the whole Descent.*

We have already seen, from the preceding observation, that the effect of the same quantity of water, descending thro' the same perpendicular space, is double, when acting by its gravity upon an overshot wheel, to what the same produces when acting by its impulse upon an undershot. It also appears, that by increasing the head from 3 inches to 11, that is, the whole descent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8,1 to 8,4, that is, as 7 : 7,26; and consequently the increase of effect as not 1-7th of the increase of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole descent, the greater will be the effect*; because it depends less upon the impulse of the head, and more upon the gravity of the water in the buckets: and if we consider how obliquely the water issuing from the head must strike the buckets, we shall not be at a loss to account for the little ad-

S 2 vantage

vantage that arifes from the impulfe thereof; and fhall immediately fee of how little confequence this impulfe is to the effect of an overfhot wheel. However, as every thing has its limits, fo has this : for thus much is defirable, *that the water fhould have fomewhat greater velocity, than the circumference of the wheel, in coming thereon*; otherwife the wheel will not only be retarded, by the buckets ftriking the water, but thereby dafhing a part of it over, fo much of the power is loft.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requifite to give the water its proper velocity is eafily computed from the common rules of hydroftatics ; and will be found much lefs than what is generally practifed.

III. *Concerning the Velocity of the Circumference of the Wheel, in order to produce the greateft Effect.*

If a body is let fall freely from the furface of the head to the bottom of the defcent, it will take a certain time in falling ; and in this cafe the whole action of gravity is fpent in giving the body a certain velocity : but if this body in falling is made to act upon fome other body, fo as to produce a mechanical effect, the falling body will be retarded ; becaufe a part of the action of gravity is then fpent in producing the effect, and the remainder only giving motion to the falling body : and therefore *the flower a body defcends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect* ; and in confequence the greater that effect may be.

If

If a ſtream of water falls into the bucket of an overſhot wheel, it is there retained till the wheel by moving round diſcharges it : of conſequence the ſlower the wheel moves, the more water each bucket will receive : ſo that what is loſt in ſpeed, is gained by the preſſure of a greater quantity of water acting in the buckets at once : and, if conſidered only in this light, the mechanical power of an overſhot wheel to produce effects will be equal, whether it moves quick or ſlow : but if we attend to what has been juſt now obſerved of the falling body, it will appear that ſo much of the action of gravity, as is employed in giving the wheel and water therein a greater velocity, muſt be ſubtracted from its preſſure upon the buckets; ſo that, tho' the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the ſame in all caſes ; yet, as each cubic inch, when the velocity is *greater* does not preſs ſo much upon the bucket as when it is *leſs*, the power of the water to produce effects will be greater in the leſs velocity than in the greater : and hence we are led to this general rule, ·*that*, cæteris paribus, *the leſs the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this doctrine, together with the limits it is ſubject to in practice, may be deduced from the foregoing ſpecimen of a ſett of experiments.

From theſe experiments it appears, that when the wheel made about 20 turns in a minute, the effect was, near upon, the greateſt. When it made 30 turns, the effect was diminiſhed about $\frac{1}{20}$ part; but that when it made 40, it was dimimiſhed about $\frac{1}{4}$; when it made leſs than 18 $\frac{1}{4}$, its motion was irregular; and
<div align="right">when</div>

when it was loaded fo as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel fhould not be diminifhed further than what will procure fome folid advantage in point of power; becaufe, *cæteris paribus*, as the motion is flower, the buckets muft be made larger; and the wheel being more loaded with water, the ftrefs upon every part of the work will be increafed in proportion : *The beft velocity for practice therefore will be fuch, as when the wheel here ufed made about* 30 *turns in a minute; that is, when the velocity of the circumference is a little more than* 3 *feet in a fecond.*

Experience confirms, that this velocity of 3 feet in a fecond is applicable to the higheft overfhot wheels, as well as the loweft; and all other parts of the work being properly adapted thereto, will produce very nearly the greateft effect poffible : however this alfo is certain from experience, that *high wheels may deviate further from this rule, before they will lofe their power, by a given aliquot part of the whole, than low ones can be admitted to do;* for a wheel of 24 feet high may move at the rate of fix feet per fecond without lofing any confiderable part of its power *; and, on the other hand, I have feen a wheel of 33 feet high, that has moved very fteadily and well with a velocity but little exceeding 2 feet.

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* The 24 feet wheel going at 6 feet in a fecond feems owing to the fmall proportion that the head (requifite to give the water the proper velocity of the wheel) bears to the whole height.

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IV. *Con-*

IV. *Concerning the Load for an Overſhot Wheel, in order that it may produce a Maximum.*

`The maximum load for an overſhot wheel, is that which reduces the circumferences of the wheel to its proper velocity*; and this will be known, by dividing the effect it ought to produce in a given time by the ſpace intended to be deſcribed by the circumference of the wheel in the ſame time : the quotient will be the reſiſtance overcome at the circumference of the wheel ; and is equal to the load required, the friction and reſiſtance of the machinery included.

V. *Concerning the greateſt poſſible Velocity of an Overſhot Wheel.*

•The greateſt velocity that the circumference of an overſhot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies ; for it is plain that the velocity of the circumference can never be greater, than to deſcribe a ſemi-circumference, while a body let fall from the top of the wheel will deſcend thro' its diameter ; nor indeed quite ſo great, as a body deſcending thro' the ſame perpendicular ſpace cannot perform the ſame in ſo ſmall a time when paſſing thro' a ſemi-circle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall thro' the diameter in one ſecond : this wheel therefore can never arrive at a velocity equal to the making one turn in two ſeconds ; but, in reality, an overſhot wheel can never come near this velocity ; for when it acquires a certain ſpeed,
the

the greateſt part of the water is prevented from en-
tering the buckets; and the reſt, at a certain point
of its deſcent, is thrown out again by the centrifugal
force. This appears to have been the caſe in the
three firſt experiments of the foregoing ſpecimen;
but as the velocity, when this begins to happen, de-
pends upon the form of the buckets, as well as other
circumſtances, *the utmoſt velocity of overſhot wheels
is not to be determined generally*: and, indeed, it is
the leſs neceſſary in practice, as it is in this circum-
ſtance incapable of producing any *mechanical effect*,
for reaſons already given.

VI. *Concerning the greateſt Load that an Overſhot Wheel can overcome.*

*The greateſt load an overſhot wheel will overcome,
confidered abſtractedly, is unlimited or infinite*: for as
the buckets may be of any given capacity, the more
the wheel is loaded, the ſlower it turns; but the
ſlower it turns, the more will the buckets be filled
with water; and conſequently tho' the diameter of
the wheel, and quantity of water expended, are both
limited, yet no reſiſtance can be aſſigned, which it is
not able to overcome: but in practice we always
meet with ſomething that prevents our getting into
infiniteſimals; for when we really go to work to build
a wheel, the buckets muſt neceſſarily be of ſome
given capacity; and conſequently *ſuch a reſiſtance
will ſtop the wheel, as is equal to the effort of all the
buckets in one ſemi-circumference filled with water*.

The ſtructure of the buckets being given, the
quantity of this effort may be aſſigned; but is not
of much conſequence to the practice, as in this caſe
alſo

alſo the wheel loſes its power; for tho' here is the exertion of gravity upon a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no *mechanical effect*, according to our definition. But, in reality, an overſhot wheel generally ceaſes to be uſeful before it is loaded to that pitch; for *when it meets with ſuch a reſiſtance as to diminiſh its velocity to a certain degree, its motion becomes irregular; yet this never happens till the velocity of the circumference is leſs than 2 feet per ſecond, where the reſiſtance is equable,* as appears not only from the preceding ſpecimen, but from experiments on larger wheels.

Scholium.

• Having now examined the different effects of the power of water, when acting by its *impulſe*, and by its *weight*, under the titles of *underſhot* and *overſhot* wheels; we might naturally proceed to examine the effects when the impulſe and weight are combined, as in the ſeveral kinds of *breaſt-wheels, &c.* but, what has been already delivered being carefully attended to, the application of the ſame principles in theſe mixt caſes will be eaſy, and reduce what I have to ſay on this head into a narrow compaſs: for all kinds of wheels where the water cannot deſcend thro' a given ſpace, unleſs the wheel moves therewith, are to be conſidered of the nature of an overſhot wheel, according to the perpendicular height that the water deſcends from; and all thoſe that receive the impulſe or ſhock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be conſidered as underſhots. And therefore a wheel, which the

water ftrikes at a certain point below the furface of the head, and after that defcends in the arch of a circle, preffing by its gravity upon the wheel; *the effect of fuch a wheel will be equal to the effect of an underfhot, whofe head is equal to the difference of level between the furface of the water in the refervoir and the point where it ftrikes the wheel, added to that of an overfhot, whofe height is equal to the difference of level, between the point where it ftrikes the wheel and the level of the tail-water.* It is here fuppofed, that the wheel receives the fhock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmoft advantage of both thefe powers; otherwife a reduction muft be made on that account.

Many obvious and confiderable improvements upon the common practice naturally offer themfelves, from a due confideration of the principles here eftablifhed, as well as many popular errors fhow themfelves in view: but as my prefent purpofe extends no farther than the laying down fuch general rules as will be found to anfwer in practice, I leave the particular application to the intelligent artift, and to the curious in thefe matters.

PART III.

On the Conftruction and Effects of WINDMILL-SAILS.

IN trying experiments on windmill-fails, the wind itfelf is too uncertain to anfwer the purpofe: we muft therefore have recourfe to an artificial wind.

This

F

W

Z

A

Inches

12   6   0

J. Smeaton delin.

Fig. 2

Fig. 1

Scale of Feet

J. Mynde. sc.
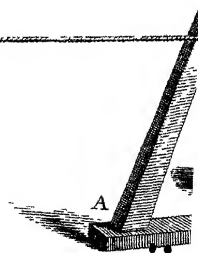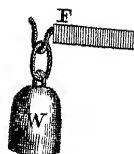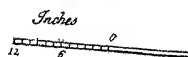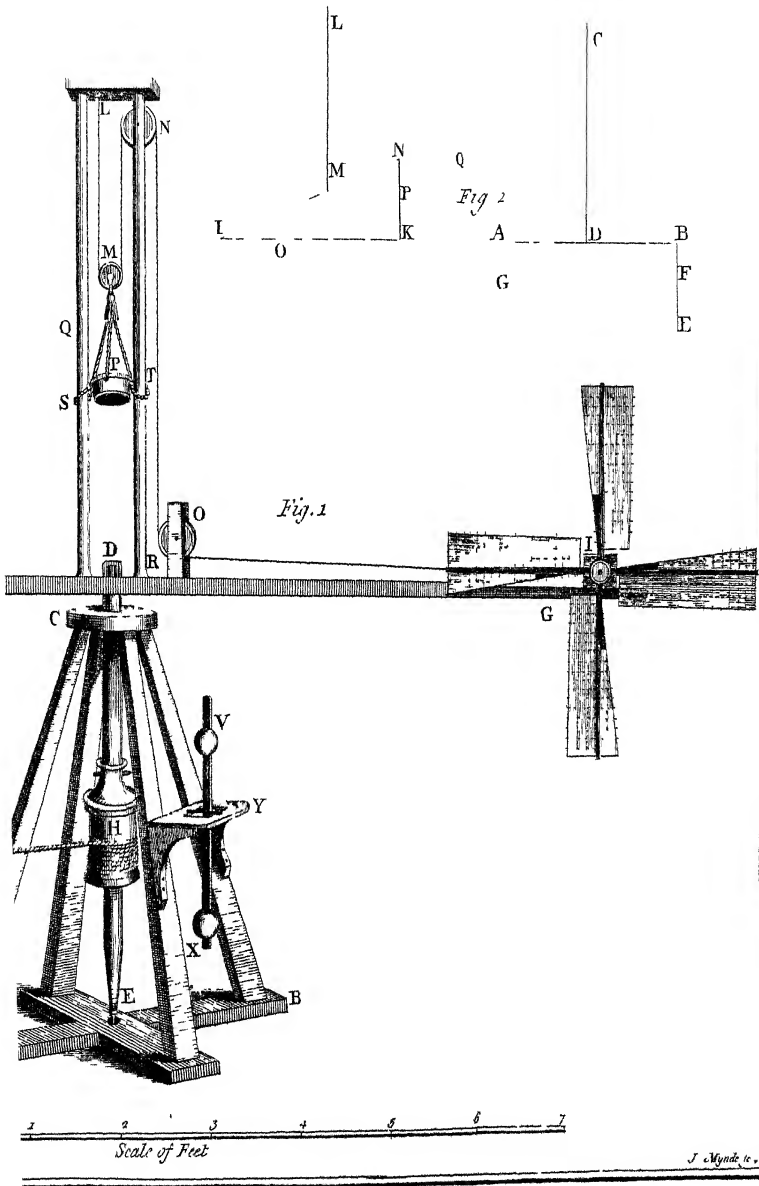
This may be done two ways; either by caufing the air to move againft the machine, or the machine to move againft the air. To caufe the air to move againft the machine, in a fufficient volumn, with fteadinefs and the requifite velocity, is not eafily put in practice: To carry the machine forward in a right line againft the air, would require a larger room than I could conveniently meet with. What I found moft practicable, therefore, was, to carry the axis, whereon the fails were to be fixed, progreffively round in the circumference of a large circle. Upon this idea * a machine was conftructed, as follows.

PLATE VI. *Fig.* I.

A B C is a pyramidical frame for fupporting the moving parts.

D E is an upright axis, whereon is framed

F G, an arm for carrying the fails at a proper dif- tance from the center of the upright axis.

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* Some years ago Mr. Roufe, an ingenious gentleman of Har-borough in Leicefterfhire, fet about trying experiments on the ve-locity of the wind, and force thereof upon plain furfaces and windmill-fails: and much about the fame time Mr. Ellicott con-trived a machine for the ufe of the late celebrated Mr. B. Robins, for trying the refiftance of plain furfaces moving thro' the air. The machines of both thefe gentlemen were much alike, tho' at that time totally unacquainted with each other's inquiries. But it often happens, that when two perfons think juftly upon the fame fubject, their experiments are alike. This machine was alfo built upon the fame idea as the foregoing; but differed in having the hand for the firft mover, with a pendulum for its regulator, inftead of a weight, as in the former; which was certainly beft for the pur-pofes of meafuring the impulfe of the wind, or refiftance of plains: but the latter is more applicable to experiments on windmill-fails; becaufe every change of pofition of the fame fails will occafion their meeting the air with a different velocity, tho' urged by the fame weight.

T 2 ·His

H is a barrel upon the upright axis, whereon is
wound a cord; which, being drawn by the
hand, gives a circular motion to the axis, and
to the arm F G; and thereby carries the axis
of the fails in the circumference of a circle,
whofe radius is D I, caufing thereby the fails
to ftrike the air, and turn round upon their
own axis.

At L is fixed the end of a fmall line, which paffing
through the pullies M N O, terminates upon a
fmall cylinder or barrel upon the axis of the
fails, and, by winding thereon, raifes

P the fcale, wherein the weights are placed for
trying the power of the fails. This fcale, moving
up and down in the direction of the upright
axis, receives no difturbance from the circular
motion.

Q R two parallel pillars ftanding upon the arm F G,
for the purpofe of fupporting and keeping
fteady the fcale P; which is kept from fwing-
ing by means of

S T two fmall chains, which hang loofely round the
two pillars.

W is a weight, for bringing the center of gravity of
the moveable part of the machine into the cen-
ter of motion of the axis D E.

V X is a pendulum, compofed of two balls of lead,
which are moveable upon a wooden rod, and
thereby can be fo adjufted, as to vibrate in any
time required. This pendulum hangs upon a
cylindrical wire, whereon it vibrates, as on a
rolling axis.

Y is a perforated table for fupporting the axis of
the pendulum.

Notes

Note, The pendulum being fo adjufted, as to make two vibrations in the time that the arm F G is intended to make one turn; the pendulum being fet a vibrating, the experimenter pulls by the cord Z, with fufficient force to make each half revolution of the arm to correfpond with each vibration, as equal as poffible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it eafy to give motion thereto with all the regularity that is neceffary.

Specimen of a Sett of Experiments.

Radius of the fails —— —— —— 2 1 inches
Length of ditto in the cloth —— —— 1 8
Breadth of ditto —— —— —— 5,6
⁎ { Angle at the extremity —— —— 1 0 degrees
⁎ { Ditto at the greateft inclination —— 2 5
2 0 turns of the fails raifed the weight 1 1, 3 inches
Velocity of the center of the fails, in the ⎫
 circumference of the great circle, in a ⎬ 6 fᵗ. 0 in.
 fecond —— —— —— —— ⎭
Continuance of the experiment —— 5 2 feconds.

Nᵒ.	Wt. in the fcale.	Turns.	Product.
1	0 *lb.*	108	0
2	6	85	510
3	6½	81	526½
4	7	78	546
5	7½	73	547¼ maximᵐ
6	8	65	520
7	9	0	0

⁎ In all the following experiments the angle of the fails is accounted from the plain of their motion; that is, when they ftand at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle fo denoted, the weather of the fail; which they denominate greater or lefs, according to the quantity of this angle.

N. B.

N. B. The weight of the fcale and pulley was 3 oz.; and that 1 oz. fufpended upon one of the radii, at 12 ½ iches from the center of the axis, juft over-came the friction fcale and load of 7 ½ lb.; and placed at 14 $\frac{19}{20}$ inches, overcame the fame refift-ances with 9 lb. in the fcale.

Reduction of the preceding Specimen.

N°. 5. being taken for the maximum, the weight in the fcale was 7 lb. 8 oz. which, with the weight of the fcale and pulley 3 oz. makes 7 lb. 11 oz. equal to 123 oz.; this added to the friction of the machinery, the fum is the whole refiftance *. The friction of the machinery is thus deduced : Since 20 turns of the fails raifed the weight 11,3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raifed by a fingle line, the radius of the cylinder being half the former, *viz.* .09, the refiftance would have been the fame : we fhall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the fmall weight was applied; fo is the weight applied to the arm, to a fourth weight, which is equivalent to the fum of the whole refiftance to-gether; that is, .09 : 12,5 :: 1 oz. : 139 oz.: this exceeds 123 oz. the weight in the fcale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8 lb. 11 oz. = 8,69 lb. for the fum of the whole re-

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* The refiftance of the air is not taken into the account of refiftance, becaufe it is infeparable from the application of the power.

fiftance;

fiftance ; and this, multiplied by 73 turns, makes a product of 634, which may be called the reprefentative of the *effect* produced.

In like manner, if the weight 9 lb. which caufed the fails to reft after being in motion, be augmented by the weight of the fcale and its relative friction, it will become 10,37 lb. The refult of this fpecimen is fet down in N°. 12. of Table III. and the refult of every other fett of experiments therein contained were made and reduced in the fame manner.

TABLE

TABLE III. *Containing Nineteen Setts of Experiments on Windmill-Sails of various Structures, Positions, and Quantities of Surfaces.*

The kind of sails made use of.	N°.	Angle at the extremities.	Greatest angle.	Turns of the sails unloaded.	Turns of ditto at the maximum.	Load at the maximum.	Greatest load.	Product.	Quantity of surface.	Ratio of greatest velocity to the velocity at a maximum.	Ratio of greatest load to the load at maximum.	Ratio of surface to the product.
						lb.	lb.		Sq. In			
Plain fails at an angle of 55°.	1	35°	35°	66	42	7,56	12,59	318	404	10:7	10:6	10:7,9
Plain fails weather'd according to the common practice.	2	12	12		70	6,3	7,56	441	404		10:8,3	10:10,1
	3	15	15	105	69	6,72	8,12	464	404	10:6,6	10:8,3	10:10,15
	4	18	18	96	66	7,0	9,81	462	404	10:7,	10:7,1	10:10,15
Weathered according to *Maclaurin's theorem.*	5	9	26½		66	7,0		462	404			10:11,4
	6	12	29½		70½	7,35		518	404			10:12,8
	7	15	32¼		63½	8,3		527	404			10:13,
Sails weathered in the *Dutch* manner, tried in various positions.	8	0	15	120	93	4,75	5,31	442	404	10:7,7	10:8,9	10:11,
	9	3	18	120	79	7,0	8,12	553	404	10:6,6	10:8,6	10:13,7
	10	5	20		78	7,5	8,12	585	404		10:9,2	10:14,5
	11	7½	22½	113	77	8,3	9,81	639	404	10:6,8	10:8,5	10:15,8
	12	10	25	108	73	8,69	10,37	634	404	10:6,8	10:8,4	10:15,7
	13	12	27	100	66	8,41	10,94	580	404	10:6,6	10:7,7	10:14,4
Sails weathered in the *Dutch* manner, but *enlarged* towards the extremities.	14	7½	22½	123	75	10,65	12,59	799	505	10:6,1	10:8,5	10:15,8
	15	10	25	117	74	11,08	13,69	820	505	10:6,3	10:8,1	10:16,2
	16	12	27	114	66	12,09	14,23	799	505	10:5,8	10:8,4	10:15,8
	17	15	30	96	63	12,09	14,78	762	505	10:6,6	10:8,2	10:15,1
8 fails being *sectors* of *ellipses* in their best positions.	18	12	22	105	64¼	16,42	27,87	1059	854	10:6,1	10:5,9	10:12,4
	19	12	22	99	64½	18,06		1165	1146	10:5,9		10:10,1
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

Observations

Obſervations and Deductions from the preceding Experiments.

I. *Concerning the beſt Form and Poſition of Wind-mill-Sails.*

In Table III. N°. 1. is contained the reſult of a ſett of experiments upon ſails ſet at the angle which the celebrated Monſ. Parint, and ſucceeding geometricians for many years, held to be the beſt; *viz.* thoſe whoſe planes make an angle 55° nearly with the axis; the complement whereof, or angle that the plane of the ſail makes with the plane of their motion, will therefore be 35°, as ſet down in col. 2. and 3. Now if we multiply their number of turns by the weight they lifted, when working to the greateſt advantage, as ſet down in columns 5. and 6. and compare this product (col. 8.) with the other products contained in the ſame column, inſtead of being the greateſt, it turns out the leaſt of all the reſt. But if we ſet the angle of the ſame planes at ſomewhat leſs than half the former, or at any angle from 15° to 18°, as in N°. 3. and 4. that is, from 72° to 75° with the axis, the product will be increaſed in the ratio of 31 : 45; and this is the angle moſt commonly made uſe of by practitioners, when the ſurfaces of the ſails are planes.

If nothing more was intended than to determine the moſt efficacious angle to make a mill acquire motion from a ſtate of reſt, or to prevent it from paſſing into reſt from a ſtate of motion, we ſhall find the poſition of N°. 1. the beſt; for if we conſult col. 7. which contains the leaſt weights, that would make the ſails paſs from motion to reſt, we ſhall find that of N°. 1.

U (relative

(relative to the quantity of cloth) the greateſt of all.
But if the ſails are intended, with given dimenſions,
to produce the greateſt effect poſſible in a given time,
we muſt intirely reject thoſe of N°. 1. and, *if we*
are confined to the uſe of planes, conform ourſelves to
ſome angle between N°. 3. *and* 4. *that is, not leſs than*
72°, or greater than 75°, with the axis.

 The late celebrated Mr. Maclaurin has judiciouſly
diſtinguiſhed between the action of the wind upon a
ſail at reſt, and a ſail in motion ; and, in conſequence,
as the motion is more rapid near the extremities than
towards the center, that the angle of the different
parts of the ſail, as they recede from the center,
ſhould be varied. For this purpoſe he has furniſhed us
with the following theorem *. " Suppoſe the velocity
" of the wind to be repreſented by *a*, and the velo-
" city of any given part of the ſail to be denoted by
" *c* ; then the effort of the wind upon that part of
" the ſail will be greateſt when the tangent of the
" angle, in which the wind ſtrikes it, is to radius as
" $\sqrt{2 + \frac{9cc}{4aa}} + \frac{3c}{2a}$ to 1." This theorem then aſ-
ſigns the law, by which the angle is to be varied ac-
cording to the velocity of each part of the ſail to the
wind : but as it is left undetermined what velocity
any one given part of the ſail ought to have in reſpect
to the wind, the angle that any one part of the ſail
ought to have, is left undetermined alſo ; ſo that we
are ſtill at a loſs for the proper *data* to apply the theo-
rem. However, being willing to avail myſelf thereof,
and conſidering that any angle from 15° to 18° was
beſt ſuited to a plane, and of conſequence the beſt

* Maclaurin's account of Sir Iſaac Newton's philoſophical diſ-
coveries, p. 176, art. 29.

mean

mean angle, I made the fail, at the middle diftance between the center and the extremity, to ftand at an angle of $15° 41'$ with the plane of the motion; in which cafe the velocity of that part of the fail, when loaded to a *maximum*, would be equal to that of the wind, or $c = a$. This being determined, the reft were inclined according to the theorem, as follows:

		Angle with the axis.	Angle of weather.
	$\frac{1}{6} - - c = \frac{1}{3} a - -$	$63° 26'$	$- - 26° 34'$
Parts of the	$\frac{2}{6} - - c = \frac{2}{3} a - -$	$69\ \ 54$	$- - 20\ \ \ 6$
radius from	$\frac{1}{2} - - c = a\ \ - - $	$74\ \ 19$	$- - 15\ \ 41$ middle
the center.	$\frac{2}{3} - - c = 1\frac{1}{3} a -$	$77\ \ 20$	$- - 12\ \ 40$
	$\frac{5}{6} - - c = 1\frac{3}{4} a -$	$79\ \ 27$	$- - 10\ \ 33$
	$1 - - c = 2 a - -$	$81\ \ \ 0$	$- - \ \ 9\ \ \ 0$ extremity.

• The refult hereof was according to N°. 5. being nearly the fame as the plane fails, in their beft pofition: but being turned round in their fockets, fo that every part of each fail ftood at an angle of 3°, and afterwards of 6°, greater than before, that is, their extremities being moved from 9° to 12° and 15°, the products were advanced to 518 and 527 refpectively. Now from the fmall difference between thofe two products, we may conclude, that they were nearly in their beft pofition, according to N°. 7. or fome angle between that and N°. 6: but from thefe, as well as the plane fails and others, we may alfo conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the beft.*

It is to be obferved, that a fail inclined by the preceding rule will expofe a convex furface to the wind: whereas the Dutch, and all our modern

mill-

mill-builders, tho' they make the angle to diminifh, in receding from the center towards the extremity, yet conftantly do it in fuch manner, as that the fur-face of the fail may be concave towards the wind. In this manner the fails made ufe of in N°. 8, 9, 10, 11, 12, and 13. were conftructed; the middle of the fail making an angle with the extreme bar of 12°; and the greateft angle (which was about ⅓ of the ra-dius from the centre) of 15° therewith. Thofe fails being tried in various pofitions, the beft appears to be that of N°. 11. where the extremities ftood at an angle of 7°½ with the plane of motion, the product being 639: greater than that of thofe made by the theorem in the ratio of 9 : 11, and double to that of N°. 1.; and this was the greateft product that could be procured without an augmentation of furface. Hence it appears, that *when the wind falls upon a concave furface, it is an advantage to the power of the whole, tho' every part, taken feparately, fhould not be difpofed to the beft advantage* *.

Having thus obtained the beft pofition of the fails, or manner of weathering, as it is called by workmen, the next point was to try what advantage could be

---

* By feveral trials in large I have found the following angles to anfwer as well as any. The radius is fuppofed to be divided into 6 parts and 1-6th, reckoning from the center, is called 1, the ex-tremity being denoted 6.

N°.	Angle with the axis.	Angle with the plane of motion.
1	72°	18°
2	71	19
3	72	18 middle.
4	74	16
5	77½	12½
6	83	7 extremity.

made

made by an addition of surface upon the same radius. For this purpose, the sails made use of had the same weather as those N°. 8. to 13, with an addition to the leading side of each of a triangular cloth, whose height was equal to the height of the sail, and whose base was equal to half the breadth : of consequence the increase of surface upon the whole was one fourth part, or as 4 : 5. Those sails, by being turned round in their sockets, were tried in four different positions, specified in N°. 14, 15, 16, and 17; from whence it appears, that the best was when every part of the sail made a greater angle by 2° ½, with the plane of the motion, than those without the addition, as appears by N°. 15. the product being 826 : this exceeds 639 more than in the ratio of 4 : 5, or that of the increase of cloth. Hence it appears, that *a broader sail requires a greater angle*; and *that when the sail is broader at the extremity, than near the center, this shape is more advantageous than that of a parallelogram* *.

Many have imagined, that the more sail, the greater the advantage, and have therefore proposed to fill up the whole area : and by making each sail a sector of an ellipsis, according to Monsieur Parint, to intercept the whole cylinder of wind, and thereby to produce the greatest effect possible.

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* The figure and proportion of the enlarged sails, which I have found best to answer in large, are represented in the figure, Plate VI. where the extreme bar is 1-3d of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular or leading sail is covered with board from the point downwards 1-3d of its height, the rest with cloth as usual. The angles of weather in the preceding note are best for the enlarged sails also ; for in practice it is found, that the sails had better have too little than too much weather.

We

We have therefore proceeded to inquire, how far the effect could be increased by a further enlargement of the surface, upon the same radius of which N°. 18 and 19 are specimens. The surfaces indeed were not made planes, and set at an angle of 35°, as Parint proposed; because, from N°. 1. we learn, that this position has nothing to do, when we intend them to work to the greatest advantage. We therefore gave them such an angle as the preceding experiments indicated for such sort of sails, *viz.* 12° at the extremity, and 22° for the greatest weather. By N°. 18 we have the product 1059, greater than N°. 15. in the ratio of 7 : 9; but then the augmentation of cloth is almost 7 : 12. By N°. 19. we have the product 1165, that is greater than N°. 15. as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; consequently had the same quantity of cloth as in N°. 18. been disposed in a figure similar to that of N°. 15, instead of the product 1059, we should have had the product 1386; and in N°. 19, instead of the product 1165, we should have had a product of 1860; as will be further made appear in the course of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crowded with sail, the less effect is produced in proportion to the surface: and by pursuing the experiments still further, I found, that tho' in N°. 19. the surface of all the sails together were not more than 7-8ths of the circular area containing them, yet a further addition rather diminished than increased the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greatest effect for want of proper interstices to escape.*

It

It is certainly defirable, that the fails of windmills fhould be as fhort as poffible; but at the fame time it is equally defirable, that the quantity of cloth fhould be the leaft that may be, to avoid damage by fudden fqualls of wind. The beft ftructure, therefore, for large mills, is that where the quantity of cloth is the greateft, in a given circle, that can be: on this condition, that the effect holds out in proportion to the quantity of cloth; for otherwife the effect can be augmented in a given degree by a leffer increafe of cloth upon a larger radius, than would be required, if the cloth was increafed upon the fame radius. The moft ufeful figure therefore for practice, is that of N°. 9. or 10. as has been experienced upon feveral mills in large.

TABLE IV. Containing the Result of six Sets of Experiments, made for determining the Difference of Effect, according to the different Velocity of the Wind.

N. B. The sails were of the same size and kind as those of N° 10, 11, and 1. Tab. IV. Continuance of the Experiment one minute.

N°	Angle at the extremity	Velocity of the wind in a second (l. m.)	Turns of the sails unloaded	Turns of the sails at maximum	Load at the maximum	Greatest load	Product	Maximum load for the half velocity	Turns of the sails therewith	Product of lesser load and greater velocity	Ratio of the two products	Ratio of the greatest velocity to the velocity at a maximum	Ratio of the greatest load to the load at a maximum
1	5°	4 4¼ 9	96	66	lb. 4,47	lb. 5,37	295					10:6,9	10:8,3
2	5	4 8	207	122	16,42	18,06	2003	4,47	180	805	10:27,3	10:5,9	10:9,1
3	7¼	4 4¼ 9		65	4,62		300						
4	7¼	4 8		130	17,52		2278	4,62	180	832	10:27,8		
5	10	4 4¼ 9	91	61	5,03	5,87	307					10:6,7	10:8,5
6	10	4 8	178	110	18,61	21,34	2047	5,03	158	795	10:26,	10:6,2	10:8,7

| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 |

5

II. *Concerning the ratio between the velocity of windmill sails unloaded, and their velocity when loaded to a maximum.*

Those ratio's, as they turned out in experiments upon different kinds of sails, and with different inclinations (the velocity of the wind being the same) are contained in column 10 of tab. III. where the extremes differ from the ratio of 10 : 7,7 to that of 10 : 5,8; but *the most general ratio of the whole will be nearly as* 3 : 2. This ratio also agrees sufficiently near with experiments where the velocity of the wind was different, as in those contained in tab. IV. col. 13. in which the ratio's differ from 10 : 6,9 to that of 10 : 5,9. However, it appears in general, that where the power is greater, whether by an enlargement of surface, or a greater velocity of the wind, that the second term of the ratio is less.

III. *Concerning the ratio between the greatest load that the sails will bear without stopping, or what is nearly the same thing, between the least load that will stop the sails, and the load at the maximum.*

Those ratio's for different kinds of sails and inclinations, are collected in col. 11. tab. III. where the extremes differ from the ratio of 10 : 6 to that of 10 : 9,2; but taking in those setts of experiments only, where the sails respectively answered best, *the ratio's will be confined between that of* 10 : 8 *and of* 10 : 9; *and at a medium about* 10 : 8,3 *or of* 6 : 5. This ratio also agrees nearly with those in col. 14 of tab. IV. However it appears, upon the whole, that in those instances, where the angle of the sails or

quantity of cloth were greateſt, that the ſecond term of the ratio was leſs.

IV. *Concerning the effects of ſails, according to the different velocity of the wind.*

Maxim 1. *The velocity of windmill ſails, whether unloaded, or loaded ſo as to produce a maximum, is nearly as the velocity of the wind, their ſhape and poſition being the ſame.*

This appears by comparing together the reſpective numbers of columns 4 and 5, tab. IV. wherein thoſe of numbers 2, 4, and 6, ought to be double of numbers 1, 3, and 5: but as the deviation is no-where greater than what may be imputed to the inaccuracy of the experiments themſelves, and hold good exactly in numbers 3 and 4; which ſetts were deduced from the medium of a number of experiments, carefully repeated the ſame day, and on that account are moſt to be depended upon; we may therefore conclude the maxim true.

Maxim 2. *The load at the maximum is nearly, but ſomewhat leſs than, as the ſquare of the velocity of the wind, the ſhape and poſition of the ſails being the ſame.*

This appears by comparing together the numbers in col. 6. tab. IV. wherein thoſe of numbers 2, 4, and 6 (as the velocity is double), ought to be quadruple of thoſe of numbers 1, 3, and 5; inſtead of which they fall ſhort, number 2 by $\frac{1}{14}$, number 4 by $\frac{1}{9}$, and number 6 by $\frac{1}{13}$ part of the whole. The greateſt of thoſe deviations is not more conſiderable than might be imputed to the unavoidable

errors

errors in making the experiments: but as those
experiments, as well as those of the greatest load, all
deviate the same way; and also coincide with some
experiments communicated to me by Mr. Rouse
upon the resistance of planes; I am led to suppose a
small deviation, whereby the load falls short of the
squares of the velocity; and since the experiments
Nº 3 and 4. are most to be depended upon, we
must conclude, that when the velocity is double, the
load falls short of its due proportion by $\frac{1}{19}$, or, for
the sake of a round number, by about $\frac{1}{20}$ part of the
whole.

Maxim 3d. *The effects of the same fails at a maxi-
mum are nearly, but somewhat less than, as the
cubes of the velocity of the wind.*

It has already been proved, Maxim 1st, that the
velocity of fails at the *maximum*, is nearly as the ve-
locity of the wind; and by Maxim 2d, that the load
at the *maximum* is nearly as the square of the same
velocity: if those two maximums would hold pre-
cisely, it would be a consequence that the effect
would be in a triplicate ratio thereof: how this
agrees with experiment will appear by comparing
together the products in col. 8. of tab. 4. wherein
those of Nº 2. 4. and 6. (the velocity of the wind
being double) ought to be octuple of those of Nº 1.
3. and 5. instead of which they fall short, Nº 2. by $\frac{1}{7}$
Nº 4. by $\frac{1}{20}$, and Nº 6. by $\frac{1}{6}$ part of the whole.
Now, if we rely on Nº 3. and 4. as the turns of the
fails are as the velocity of the wind; and since the
load of the maximum falls short of the square of the
velocity by about $\frac{1}{20}$ part of the whole: the product

made

made by the multiplication of the turns into the load, muft alfo fall fhort of the triplicate ratio by about $\frac{1}{20}$ part of the whole product.

Maxim 4th. *The load of the fame fails at the maximum is nearly as the fquares, and their effect as the cubes, of their number of turns in a given time.*

This maxim may be efteemed a confequence of the three preceding ; for if the turns of the fails are as the velocity of the wind, whatever quantities are in any given ratio of the velocity of the wind, will be in the fame given ratio of the turns of the fails : and therefore, if the load at the *maximum* is as the fquare, or the effect as the cube, of the velocity of the wind, wanting $\frac{1}{20}$ part when the velocity is double ; the load at the *maximum* will alfo be as the fquare, and the effect as the cube, of the number of turns of the fails in a given time, wanting in like manner $\frac{1}{20}$ part when the number of turns are double in the fame time. In the prefent cafe, if we compare the loads at the *maximum* col. 6. with the fquares of the number of turns col. 5. of N° 1 and 2. 5 and 6. or the products of the fame numbers col. 8. with the cubes of the number of turns col. 5. inftead of falling fhort, as N° 3 and 4. they exceed thofe ratios : but as the fetts of experiments N° 1 and 2. 5 and 6. are not to be efteemed of equal authority with thofe of N° 3 and 4. we muft not rely upon them further than to obferve, that *in comparing the grofs effects of large machines, the direct proportion of the fquares and cubes refpectively, will hold as near as the effects themfelves can be obferved* ; and there-fore

fore be fufficient for practical eftimation, without any allowance.

Maxim 5th. *When fails are loaded fo as to produce a maximum at a given velocity, and the velocity of the wind increafes, the load continuing the fame; 1ftly, The increafe of effect, when the increafe of the velocity of the wind is fmall, will be nearly as the fquares of thofe velocities: 2dly, When the velocity of the wind is double, the effects will be nearly as* 10:27½*: But,* 3dly, *When the velocities compared, are more than double of that where the given load produces a maximum, the effects increafe nearly in a fimple ratio, of the velocity of the wind.*

It has already been proved, maxim 1ft and 2d, that when the velocity of the wind is increafed, the turns of the fails will increafe in the fame proportion, even when oppofed by a load as the fquare of the ve-locity; and therefore if wanting the oppofition of an increafe of load, as the fquare of the velocity, the turns of the fails will again be increafed in a fimple ratio of the velocity of the wind on that account alfo; that is, the load continuing the fame, the turns of the fails in a given time will be as the fquare of the velocity of the wind; and the effect, being in this cafe. as the turns of the fails, will be as the fquare of the velocity of the wind alfo; but this muft be under-ftood only of the firft increments of the velocity of the wind: for,

2dly, As the fails will never acquire above a given velocity in relation to the wind, tho' the load was diminifhed to nothing; when the load continues the fame,

fame, the more the velocity of the wind increafes (tho' the effect will continue to increafe) yet the more it will fall fhort of the fquare of the velocity of the wind; fo that when the velocity of the wind is double, the increafe of effect, inftead of being as 1 : 4, according to the fquares, it turns out as $10 : 27\frac{1}{2}$, as thus appears. In tab. 4. col. 9. the loads of N°. 2, 4, and 6. are the fame as the maximum loads in col. 6. of N°. 1, 3, and 5. The number of turns of the fails with thofe loads, when the velocity of the wind is double, are fet down in col. 10. and the products of their multiplification in col. 11 : thofe being compared with the products of N°. 1, 3, and 5. col. 8. furnifh the ratios fet down in col. 12. which at a medium (due regard being had to N°. 3 and 4.) will be nearly as $10 : 27\frac{1}{2}$. 3dly. The load continuing the. fame, grows more and more inconfiderable, refpecting the power of the wind as it increafes in velocity; fo that the turns of the fails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the fimple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the fails, when loaded to a maximum, will be double alfo; but, *unloaded*, will be no more than triple, by deduction 2d : and therefore the product could not have increafed beyond the ratio of 10:30 (inftead of $10 : 27\frac{1}{2}$) even fuppofing the fails not to have been retarded at all by carrying the maximum load for the half velocity. Hence we fee, that when the velocity of the wind exceeds the double of that, where a conftant load produces a maximum, that the increafe of effect, which follows the increafe of the velocity of the fails, will be nearly as the velocity

city of the wind, and ultimately in that ratio precisely. Hence alfo we fee that windmills, fuch as the different fpecies for raifing water for drainage, &c. lofe much of their full effect, when acting againft one invariable oppofition.

V. *Concerning the effects of fails of different magnitudes, the ftructure and pofition being fimilar, and the velocity of the wind the fame.*

Maxim 6. *In fails of a fimilar figure and pofition, the number of turns in a given time will be reciprocally as the radius or length of the fail.*

The extreme bar having the fame inclination to the plain of its motion, and to the wind; its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and therefore, whatever be the radius, the abfolute velocity of the extremity of the fail will be the fame: and this will hold good refpecting any other bar, whofe inclination is the fame, at a proportionable diftance from the center; it therefore follows, that the extremity of all fimilar fails, with the fame wind, will have the fame abfolute velocity; and therefore take a fpace of time to perform one revolution in proportion to the radius; or, which is the fame thing, the number of revolutions in the fame given time, will be reciprocally as the length of the fail.

Maxim 7. *The load at a maximum that fails of a fimilar figure and pofition will overcome, at a given diftance from the center of motion, will be as the cube of the radius.*

Geo-

Geometry informs us, that in similar figures the surfaces are as the squares of their similar sides; of consequence the quantity of cloth will be as the square of the radius: also in similar figures and positions, the impulse of the wind, upon every similar section of the cloth, will be in proportion to the surface of that section; and consequently, the impulse of the wind upon the whole, will be as the surface of the whole: but as the distance of every similar section, from the center of motion, will be as the radius; the distance of the center of power of the whole, from the center of motion, will be as the radius also; that is, the lever by which the power acts, will be as the radius: as therefore the impulse of the wind, respecting the quantity of cloth, is as the square of the radius, and the lever, by which it acts, as the radius simply; it follows, that the load which the sails will overcome, at a given distance from the center, will be as the cube of the radius.

Maxim 8. *The effect of sails of similar figure and position, are as the square of the radius.*

By maxim 6, it is proved, that the number of revolutions made in a given time, are as the radius inversely. Under maxim 7. it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore these equal and opposite ratios destroy one another: but as in similar figures the quantity of cloth is as the square of the radius, and the action of the wind is in proportion to the quantity of cloth, as also appears under maxim 7; it follows that the effect is as the square of the radius.

CoroL.

COROL. 1. Hence it follows, that augmenting the length of the fail, without augmenting the quantity of cloth, does is not increafe the power; becaufe what is gained by the length of the lever, is loft by the flownefs of the rotation.

COROL. 2. If fails are increafed in length, the breadth remaining the fame, the effect will be as the radius.

VI. *Concerning the velocity of the extremities of windmill fails, in refpect to the velocity of the wind.*

Maxim 9. *The velocity of the extremities of* Dutch *fails, as well as of the enlarged fails, in all their ufual pofitions when unloaded, or even loaded to a maximum, are confiderably quicker than the velocity of the wind.*

The *Dutch* fails unloaded, as in Tab. 3. N⁰ 8. made 120 revolutions in 52″: the diameter of the fails being 3 feet 6 inches, the velocity of their extremities will be 25,4 feet in a fecond; but the velocity of the wind producing it, being 6 feet in the fame time, we fhall have 6 : 25,4 : : 1 : 4,2 ; in this cafe therefore, the velocity of their extremities was 4,2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the fame fails, when loaded to a *maximum*, making then 93 turns in 52″, will be found to be as 1 : 3,3 ; or 3,3 times quicker than that of the wind.

The following table contains 6 examples of *Dutch* sails, and 4 examples of the enlarged sails, in different positions, but with the constant velocity of the wind of 6 feet in a second, from table 3 : and also 6 examples of *Dutch* sails in different positions, with different velocities of the wind, from table 4.

TABLE V. *containing the ratio of the velocity of the extremities of windmill sails to the velocity of the wind.*

N°	N° of Tab. III and IV.	Angle at the Extremity.	Velocity of the wind in a second.	Ratio of the velocity of the wind and extremities of the sails.		
				unloaded.	loaded.	
1	8	0°	6^f 0^{in}	1 : 4,2	1 : 3,3	
2	9	3	6 0	1 : 4,2	1 : 2,8	
3	10	5	6 0	— — —	1 : 2,75	
4	11	7½	6 0	1 : 4,	1 : 2,7	From Table III.
5	12	10	6 0	1 : 3,8	1 : 2,6	
6	13	12	6 0	1 : 3,5	1 : 2,3	
7	14	7½	6 0	1 : 4,3	1 : 2,6	
8	15	10	6 0	1 : 4,1	1 : 2,6	
9	16	12	6 0	1 : 4,	1 : 2,3	
10	17	15	6 0	1 : 3,35	1 : 2,2	
11	1	5	4 4½	1 : 4,	1 2,8	
12	2	5	8 9	1 : 4,3	1 2,6	
13	3	7½	4 4½	— — —	1 : 2,8	From Tab. IV.
14	4	7½	8 9	— — —	1 : 2,7	
15	5	10	4 4½	1 : 3,8	1 2,6	
16	6	10	8 9	1 : 3,4	1 : 2 3	
1	2	3	4	5	6	

It

It appears from the preceding collection of examples, that when the extremities of the *Dutch* sails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in *England*, that their velocity, unloaded, is above 4 times, and loaded to a *maximum*, above 3 times greater than that of the wind : but that when the Dutch sails, or enlarged sails, are in their best positions, their velocity unloaded is 4 times, and loaded to a *maximum*, at a medium the Dutch sails are 2,7, and the enlarged sails 2,6 times greater than the velocity of the wind. Hence we are furnished with a method of knowing the velocity of the wind, from observing the velocity of the windmill sails; for knowing the radius, and the number of turns in a minute, we shall have the velocity of the extremities; which, divided by the following divisors, will give the velocity of the wind.

Dutch sails in their common position $\begin{cases} \text{unloaded } 4.2 \\ \text{loaded } -3.3 \end{cases}$

Dutch sails in their best position -- $\begin{cases} \text{unloaded } 4.0 \\ \text{loaded } -2.7 \end{cases}$

Enlarged sails in their best position $\begin{cases} \text{unloaded } 4.0 \\ \text{loaded} - 2.6 \end{cases}$

From the above divisors there arises the following compendiums ; supposing the radius to be 30 feet, which is the most usual length in this country, and the mill to be loaded to a *maximum*, as is usually the case with corn mills ; for every 3 *turns in a minute, of the Dutch sails in their common position, the wind will move at the rate of* 2 *miles an hour* ; for every 5 *turns in a minute, of the Dutch sails in their best*

Y 2   *position,*

poſition, the wind moves 4 miles an hour; and for
every 6 *turns in a minute, of the enlarged ſails in
their beſt poſition, the wind will move* 5 *miles an*

The following table, which was communicated
to me by my friend Mr. Rouſe, and which appears to
have been conſtructed with great care, from a con-
ſiderable number of facts and experiments, and which
having relation to the ſubject of this article; I here
inſert it as he ſent it to me: but at the ſame time muſt
obſerve, that the evidence for thoſe numbers where
the velocity of the wind exceeds 50 miles an hour,
do not ſeem of equal authority with thoſe of 50 miles
an hour and under. It is alſo to be obſerved, that
the numbers in col. 3. are calculated according to the
ſquare of the velocity of the wind, which, in mode-
rate velocities, from what has been before obſerved,
will hold very nearly.

TABLE VI. *containing the velocity and force of wind, according to their common appellations.*

Velocity of the Wind.		Perpendicular force on one foot area in pounds averdupois.	Common appellations of the force of winds.
Miles in one Hour.	Feet in one fecond.		
1	1,47	,005	Hardly perceptible.
2	2,93	,020	} Juſt perceptible.
3	4,40	,044	
4	5,87	,079	} Gentle pleaſant wind.
5	7,33	,123	
10	14,67	,492	} Pleaſant briſk gale.
15	22,00	1,107	
20	29,34	1,968	} Very briſk.
25	36,67	3,075	
30	44,01	4,429	} High winds.
35	51,34	6,027	
40	58,68	7,873	} Very high.
45	66,01	9,963	
50	73,35	12,300	A ſtorm or tempeſt.
60	88,02	17,715	A great ſtorm.
80	117,36	31,490	An hurricane.
100	146,70	49,200	An hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII. *concerning the abſolute effeẛt, produced by a given velocity of the wind, upon ſails of a given magnitude and conſtruẛtion.*

It has been obſerved by praẛtitioners, that in mills with Dutch ſails in the common poſition, that when they make about 13 turns in a minute, they then

work at a mean rate: that is, by the compendiums in the laſt article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a ſecond; which, in common phraſe, would be called a *freſh gale*.

The experiments ſet down in Tab. IV. Nº 4. were tried with a wind, whoſe velocity was $8\frac{3}{4}$ feet in a ſecond; conſequently had thoſe experiments been tried with a wind, whoſe velocity was 12^3 feet in a ſecond, the effect, by maxim 3d, would have been 3 times greater; becauſe the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{3}{4}$.

From Tab. IV. Nº 4. we find, that the ſails, when the velocity of the wind was $8\frac{3}{4}$ feet in a ſecond, made 130 revolutions in a minute, with a load of 17,25 ℔. From the meaſures of the machine, preceding the ſpecimen of a ſett of experiments, we find, that 20 revolutions of the ſails raiſed the ſcale and weight 11,3 inches: 130 revolutions will therefore raiſe the ſcale 73,45 inches, which, multiplied by 17,52 ℔, makes a product of 1287, for the effect of the Dutch ſails in their beſt poſition; that is, when the velocity of the wind is $8\frac{3}{4}$ feet in a ſecond: this product therefore multiplied by three, will give 3861 for the effect of the ſame ſails, when the velocity of the wind is $12\frac{2}{3}$ feet in a ſecond.

Deſaguliers makes the utmoſt power of a man, when working ſo as to be able to hold it for ſome hours, to be equal to that of raiſing an hogſhead of water 10 feet high in a minute. Now, an hogſhead conſiſting of 63 ale gallons, being reduced into pounds averdupois, and the height into inches; the product made by multiplying thoſe two numbers will be 76800; which is 19 times greater than the

pro-

product of the fails laft-mentioned, at 12½ feet in a fecond : therefore, by maxim 8th, if we multiply the fquare root of 19, that is 4,46, by 21 inches, the length of the fail producing the effect 3861, we fhall have 93,66 inches, or 7 feet 9⅔ inches for the radius of a Dutch fail in its beft pofition, whofe mean power fhall be equal to that of a man : but if they are in their common pofition, their length muft be increafed in the ratio of the fquare root of 442 to that of 639, as thus appears ;

The ratio of the *maximum* products of N° 8 and 11. Tab. III. are as 442 : 639 ; but by maxim 8, the effects of fails of different radii are as the fquare of the radii ; confequently the the fquare roots of the products or effects, are as the radii fimply ; and therefore as the fquare root of 442 is to that of 639 ; fo is 93,66 to 112,66 ; or 9 feet 4⅔ inches.

If the fails are of the enlarged kind, then from Tab. III. N° 11 and 15. we fhall have the fquare root of 820 to that of 639 : : 93,66 : 82,8 inches, or 6 feet 10¼ inches : fo that in round numbers we fhall have the radius of a fail, of a fimilar figure to their refpective models, whofe mean power fhall be equal to that of a man ;

The Dutch fails in their common pofition   9½ feet.
The Dutch fails in their beft pofition ——— 8
The enlarged fails in their beft pofition — 7

Suppofe now the radius of a fail to be 30 feet, and to be conftructed upon the model of the enlarged fails, N° 14 or 15. Tab. III. dividing 30 by 7 we, fhall have 4,28, the fquare of which is 18,3 ; and this, according to maxim 7, will be the relative

3   power

power of a fail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet fail will be equal to the power of 18,3 men, or of $3\frac{2}{3}$ horfes; reckoning 5 men to a horfe: whereas the effect of the common Dutch fails, of the fame length, being lefs in the proportion of 820 : 442, will be fcarce equal to the power of 10 men, or of 2 horfes.

That thefe computations are not merely fpeculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged fails of 30 feet, applied to the crufhing of rape feed, by means of two runners upon the edge, for making oil; I obferved, that when the fails made 11 turns in a minute, in which cafe the velocity of the wind was about 13 feet in a fecond, according to article 6th, that the runners then made 7 turns in a minute: whereas 2 horfes, applied to the fame 2 runners, fcarcely worked them at the rate of $3\frac{1}{2}$ turns in the fame time. Laftly, with regard to the real fuperiority of the enlarged fails, above the Dutch fails as commonly made, it has fufficiently appeared, not only in thofe cafes where they have been applied to new mills, but where they have been fubftituted in the place of the others.

VIII. *Concerning horizontal windmills and water-wheels, with oblique vanes.*

Obfervations upon the effects of common windmills with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulfe, like a fhip failing before the wind, it would

be

be a very great improvement in point of power: while others attending to the extraordinary and even unexpected effects of oblique vanes, have been led to imagine, that oblique vanes applied to water-mills, would as much exceed the common water wheels, as the vertical windmills are found to have exceeded all attempts towards an horizontal one. Both these notions, but especially the first, have so plausible an appearance, that of late years there has seldom been wanting those, who have assiduously employed themselves to bring to bear designs of this kind: it may not therefore be unacceptable to endeavour to set this matter in a clear light.

PLATE VI. fig 2d. Let A B be the section of a plain, upon which let the wind blow in the direction C D, with such a velocity as to describe a given space B E, in a given time (suppose 1 second); and let A B be moved parallel to itself, in the direction C D. Now, if the plane A B moves with the same velocity as the wind; that is, if the point B moves thro' the space B E in the same time that a particle of air would move thro' the same space; it is plain that, in this case, there can be no pressure or impulse of the wind upon the plane : but if the plane moves slower than the wind, in the same direction, so that the point B may move to F, while a particle of air, setting out from B at the same instant, would move to E, then B F will express the velocity of the plane; and the relative velocity of the wind and plane will be expressed by the line F E. Let the ratio of F E to B E be given (suppose 2 : 3.); let the line A B represent the impulse of the wind upon the plane A B, when acting with its whole velocity B E; but,

when acting with its relative velocity F E, let its impulse be denoted by some aliquot part of A B, as for instance $\frac{4}{9}$ A B : then will $\frac{4}{9}$ of the parallelogram A F, represent the mechanical power of the plane ; that is, $\frac{4}{9}$ A B × $\frac{1}{3}$ B E.

2dly, Let I N be the section of a plane, inclined in such a manner, that the base I K of the rectangle triangle I K N may be equal to A B ; and the perpendicular N K=B E ; let the plane I N be struck by the wind, in the direction L M, perpendicular to I K : then, according to the known rules of oblique forces, the impulse of the wind upon the plain I N, tending to move it according to the direction L M, or N K, will be denoted by the base I K ; and that part of the impulse, tending to move it according to the direction I K, will be expressed by the perpendicular N K. Let the plane I N be moveable in the direction of I K only ; that is, the point I in the direction of I K, and the point N in the direction N Q, parallel thereto. Now it is evident, that if the point I moves thro' the line I K, while a particle of air, setting forwards at the same time from the point N, moves thro' the line N K, they will both arrive at the point K at the same time ; and consequently, in this case also, there can be no pressure or impulse of the particle of the air upon the plane I N. Now let I O be to I K as B F to B E ; and let the plane I N move at such a rate, that the point I may arrive at O, and acquire the position I Q, in the same time that a particle of wind would move thro' the space N K : as O Q is parallel to I N ; (by the properties of similar triangles) it will cut N K in the point P, in such a manner, that N P=B F, and P K=F E : hence it

<div align="right">appears,</div>

appears, that the plane I N, by acquiring the pofi-
tion O Q, withdraws itfelf from the action of the
wind, by the fame fpace N P, that the plane A B
does by acquiring the pofition F G; and confequently,
from the equality of P K to F E, the relative im-
pulfe of the wind P K, upon the plane O Q, will be
equal to the relative impulfe of the wind F E, upon
the plane F G: and fince the impulfe of the wind
upon A B, with the relative velocity F E, in the di-
rection B E, is reprefented by $\frac{4}{9}$ A B; the relative
impulfe of the wind upon the plane I N, in the di-
rection N K, will in like manner be reprefented by
$\frac{4}{9}$ I K; and the impulfe of the wind upon the plane
I N, with the relative velocity P K, in the direction
I K, will be reprefented by $\frac{4}{9}$ N K: and confequently
the mechanical power of the plane I N, in the direc-
tion I K, will be $\frac{4}{9}$ the parallellogram I Q: that is
$\frac{1}{3}$ I K $\times \frac{4}{9}$ N K: that is, from the equality of I K=A B
and N K=B E, we fhall have $\frac{4}{9}$ I Q $=\frac{1}{3}$ A B $\times \frac{4}{9}$ B E
$=\frac{4}{9}$ A B $\times \frac{1}{3}$ B E $=\frac{4}{9}$ of the area of the parallellogram
A F.. Hence we deduce this

GENERAL PROPOSITION,

*That all planes, however fituated, that intercept
the fame fection of the wind, and having the fame re-
lative velocity, in regard to the wind, when reduced
into the fame direction, have equal powers to produce
mechanical effects.*

For what is loft by the obliquity of the impulfe,
is gained by the velocity of the motion.

Hence it appears, that an oblique fail is under no
difadvantage in refpect of power, compared with a
direct one; except what arifes from a diminution of

its

irs breadth, in refpect to the fection of the wind: the breadth I N being by obliquity reduced to I K.

The difadvantage of horizontal windmills therefore does not confift in this; that each fail, when directly expofed to the wind, is capable of a lefs power, than an oblique one of the fame dimenfions; but that in an horizontal windmill, little more than one fail can be acting at once: whereas in the common windmill, all the four act together: and therefore, fuppofing each vane of an horizontal windmill, of the fame dimenfions as each vane of the vertical, it is manifeft the power of a vertical mill with four fails, will be four times greater than the power of the horizontal one, let its number of vanes be what it will: this difadvantage arifes from the nature of the thing; but if we confider the further difadvantage, that arifes from the difficulty of getting the fails back again againft the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common fort; as has appeared in fome attempts of this kind.

In like manner, as little improvement is to be expected from water-mills with oblique vanes: for the power of the fame fection of a ftream of water, is not greater when acting upon an oblique vane, than when acting upon a direct one: and any advantage that can be made by intercepting a greater fection, which fometimes may be done in the cafe of an open river, will be counterballanced by the fuperior refiftance, that fuch vanes would meet with by moving at right angles to the current: whereas the common floats always move with the water nearly in the fame direction.

Here

Here it may reasonably be asked, that since our geometrical demonstration is general, and proves, that one angle of obliquity is as good as another; why in our experiments it appears, that there is a certain angle which is to be preferred to all the rest? It is to be observed, that if the breadth of the sail I N is given, the greater the angle K I N, and the less will be the base I K : that is, the section of wind intersected, will be less : on the other hand, the more acute the angle K I N, the less will be the perpendicular K N : that is, the impulse of the wind, in the direction I K being less, and the velocity of the sail greater ; the resistance of the medium will be greater also. Hence therefore, as there is a diminution of the section of the wind intercepted on one hand, and an increase of resistance on the other, there is some angle, where the disadvantage arising from these causes upon the whole is the least of all ; but as the disadvantage arising from resistance is more of a physical than geometrical consideration, the true angle will best be assigned by experiment.

SCHOLIUM.

In trying the experiments contained in Tab. III. and IV. the different specific gravity of the air, which is undoubtedly different at different times, will cause a difference in the load, proportional to the difference of its specific gravity, tho' its velocity remains the same ; and a variation of specific gravity may arise not only from a variation of the weight of the whole column, but also by the difference of heat of the air concerned in the experiment, and possibly of other causes ; yet the irregularities that might arise from a
dif-

difference of fpecific gravity were thought to be too fmall to be perceivable, till after the principal experiments were made, and their effects compared ; from which, as well as fucceeding experiments, thofe variations were found to be capable of producing a fenfible, tho' no very confiderable effect: however, as all the experiments were tried in the fummer fea-fon, in the day-time, and under cover ; we may fup-pofe that the principal fource of error would arife from the different weight of the column of the atmo-fphere at different times ; but as this feldom varies above $\frac{1}{17}$ part of the whole, we may conclude, that tho' many of the irregularities contained in the experi-ments referred to in the foregoing effay, might arife from this caufe ; yet as all the principal conclufions are drawn from the medium of a confiderable num-ber, many whereof were made at different times, it is prefumed that they will nearly agree with the truth, and be altogether fufficient for regulating the practical conftruction of thofe kind of machines, for which ufe they were principally intended.

XX. *An*

XIX. *An Account of the remarkable Altera-*
tion of Colour in a Negro Woman : In a
Letter to the Reverend Mr. Alexander
Williamfon *of* Maryland, *from Mr.*
James Bate, *Surgeon in that Province.*
Communicated by Alexander Ruffel, *M. D.*
F. R. S.

To the Rev. Tho. Birch, *D. D. Secret. R. S.*

S I R,

Read, 10 May, 1759. SOME account of the extraordinary facts mentioned in the inclofed letter having been communicated to me, above a year ago, by the Reverend and ingenious Mr. Williamfon of Maryland, I thought them worthy of attention; and therefore begged, that he would be fo kind, as to get fome Gentlemen of the faculty to affift him in making a further enquiry. He has accordingly tranfmitted to me the cafe, now fent you, as related by Mr. Bate, a praÖitioner in phyfic, of fome note in that part of the world.

I have alfo had the particulars fince confirmed to me by two Gentlemen now in England, who have often feen the woman; fo fhall make no farther apology for giving you this trouble, in order to its being communicated to the Royal Society.

I am, Sir,

Your moft obedient humble Servant,

Limeftreet, May 8th, 1759.

Alexander Ruffell.

S I R,

SIR,

IN compliance with your defire, I fend as particu-
lar an account of the extraordinary metamorpho-
fis obfervable in colonel Barnes's negro woman, as I
have been able to procure.

Frank, a cook-maid of the above-named Gentle-
man, a native of Virginia, about 40 years of age,
remarkably healthy, of a ftrong and robuft confti-
tution, had her fkin originally as dark as that of the
moft fwarthy African; but about fifteen years ago,
obferved that membrane, in the parts next adjoining
to the finger nails, to become white. Her mouth
foon underwent the fame change, and the phæno-
menon hath fince continued gradually to extend it-
felf over the whole body; fo that every part of its
furface is become more or lefs the fubject of this
furprifing alteration. In her prefent ftate four parts
in five of the fkin are white, fmooth, and tranfpa-
rent, as in a fair European, elegantly fhewing the
ramifications of the fubjacent blood-veffels : the
parts remaining footy daily lofe their blacknefs, and
in fome meafure partake of the prevailing colour;
fo that a very few years will, in all probability, in-
duce a total change. The neck and back, along
the courfe of the vertebræ, maintain their priftine
hue the moft, and in fome fpots proclaim their ori-
ginal ftate: the head, face, and breaft, with belly,
legs, arms, and thighs, are almoft wholly white;
the pudenda and axillæ party-coloured; the fkin of
thefe parts, as far as white, being covered with white
hair; where dark, with black. Her face and breaft,
as often as the paffions anger, fhame, &c. have been
3   excited.

excited in her, have been immediately obferved to glow with blufhes; as alfo when, in purfuance of her bufinefs, fhe has been expofed to the action of the fire upon thefe paits, fome freckles have made their appearance. After having defcribed her prefent appearance, as well as I am able, I fhall not pretend to offer any conjectures of my own upon the fubject; left being led away by a train of reafoning, I fhould lofe myfelf, in endeavouring to eftablifh a favourite hypothefis; but, on the contrary, fhall confine myfelf to a fimple narration of fuch facts, as may prevent miftakes, or obviate difficulties, arifing in the inveftigation of this difficult piece of phyfical hiftory. And in the firft place, left the change fhould be thought the confequence of a previous morbid ftate, fhe declares, that, excepting about feventeen years ago, when fhe was delivered of a child, fhe hath never been afflicted by any complaint of 24 hours continuance; and that fhe never remembers the catamenia to have been either irregular or obftructed, only during this pregnancy: fhe hath never been fubject to any cutaneous diforders, or made ufe of any external applications, by which this phænomenon might be produced. The effects of the bile upon the fkin are well known to phyficians, and have given rife to an opinion, that its colour was determined thereby: for my own part I cannot believe it has any thing to do here, fince, from all the circumftances I have been able to collect, I cannot find the leaft reafon to fufpect, that this fluid, whether cyftic or hepatic, has undergone any alteration. As uftion is known to make the fkin of negroes become white, and as fhe is daily employed in the bufineffes

of cookery, it may perhaps be fuppofed the effect of heat : but this can never be the cafe, as fhe has ever been well clad ; and the change is as obvious in the parts protected from the action of that element, as in thofe the moft expofed thereto. As an emunctory, the fkin feems to perform its office as well as poffible, the fweat with the greateft freedom indifferently pervading the black and white parts. The effects of a blifter I mentioned to you I am yet a ftranger to, as that, which I applied upon the outfide of the arm, did not anfwer the intended purpofe : whether this was owing to its being laid upon a part too much expofed, or that the corpus reticulare being deftroyed, there may be fuch an adhefion of the cuticle to the cutis, as may render them infeparable, a fecond experiment muft determine. If, upon your fending this to Dr. Ruffell, he, or any of his learned acquaintance, to whom he may communicate it, fhall think any future experiments neceffary, I fhall be glad to execute them under their directions, not only for my private fatisfaction, but in order to convince you, how much pleafure I take in doing every thing, that may oblige Mr. Williamfon or his friends.

I am, Sir,

Your obedient humble Servant,

Leonard-Town,
6th Sept. 1758.

James Bate,
Surgeon in Maryland.

XX. *The*

XX. *The Cafe of a paralytic Patient cured by an electrical Application, inclofed in a Letter from Doctor* Himfel, *at* Riga, *to* Jacob de Caftro Sarmento, *M. D. F. R. S. Tranflated from the* French.

ON the 10th of March 1752, a young man 20 years of age, whofe name is *Mauve*, in his infancy had a fall, by which his right arm became paralytic, of which he never had the leaft ufe from five years old. He was brought to me in order to attempt his cure by electricity; and he was in much the fame condition with the patient cured by Mr. *Jalabert* at Geneva; who could not move his right hand in the leaft, and was afterwards able to help himfelf, and follow his trade. All the fingers of the paralytic hand were difabled, and the hand was fo bent towards the elbow, as to form a right angle; the hand was of a red and blue colour, as if it had been ftruck with froft. The extenfor mufcles of the wrift and fingers had an *atrophy*, and the whole fore-arm was fhrivled; befides which, all his right fide was paralytic, as well as the right arm, at the beginning of his difeafe; and the right foot was very much weakened.

He approached the electrical tube, and touched it for fome minutes; till the *thenar, hypothenar, antithenar, indicator*, and the *interoffei*, as well as the fingers of the lame hand, fuffered electrical fhocks one after another.

The

The fpectators were amazed to find, that, at this firft trial, the young man could fucceffively extend his *thumb*, *index*, and *middle fingers*, and at length the ring and little fingers; and on the following days his friends obferved, that his hand was no longer fo rigid as it had been. The good effect of this firft effay, and the defire of the patient and his relations, encouraged me to repeat it on the 16th; and I increafed the electrical power by the known means of the flagon of water. I ftripped his arm, and by turns gave the fhock to the *cubitæus externus*, *radius externus*, the *extenfor magnus*, the *interoffei*, *extenfor policis*, and the other mufcles of the thumb. He complained fometimes, that his hand and arm were numbed and ftiff: I therefore caufed it to be well rubbed with a towel, by which it became flexible: I made them alfo rub his hand with a woollen cloth, and continue it fome time; and I obferved, that this hand, which bent towards the elbow for fifteen years, became ftraitened out, altho' it fell into its former fituation fome feconds after. This was the more remarkable, as many applications had been made before by the ableft phyficians. The 17th the patient told me, that his hand opened of itfelf the day before at three different times, and was alfo ftraitened out with the elbow; but that it clofed and bent up again of itfelf. The fecond time that I electrified him, he was more fenfible of the fparks, and this day he felt them ftill more, having made them ftronger by Mufchenbroek's invention; he could fcarce bear them: the numbnefs of his arm and hand followed as before, which generally happened afterwards upon electrifying him; but he always found

him-

himſelf better upon rubbing, as before. This third time he extended his hand and fingers more than once, without the affiſtance of his other hand : nevertheleſs, as he complained of very ſharp blows from the ſparks, I changed my firſt method the 18th, and mounting upon a box filled with pitch, I took the tube in one hand, and, by means of a key which I held in the other, I touched him where I thought proper, in order to draw the ſparks ; but as the ſhocks were very ſtrong, I placed under the tube two flagons filled with water, making a communication of the water with the tube, by means of an iron wire. In viewing the naked arm, it plainly appeared it was become more fleſhy. The night after he ſlept better than the foregoing between the 17th and 18th, when, tho' he found himſelf much fatigued all over, his ſleep was often interrupted by very ſharp pains in the upper part of his arm; which part, nevertheleſs, I had not touched at all ; but he had that day undergone very ſtrong ſhocks. He alſo felt great heat in his hand together with theſe pains, and this heat was felt almoſt every time after he had been electrified, both in the hand and the whole length of the arm.

On the 19th, 20th, and 21ſt, ſome affairs prevented my continuing the operation; but I adviſed him to exerciſe his arm every way he poſſibly could, having obſerved, that even the muſcles of the arm leaſt affected, after ſo long a time of inactivity, were much weakened ; and the patient was gradually capable of taking up his glove from the ground ſeveral times ſucceſſively, and even to put on and take off his hat with the paralytic hand, which he alſo repeated the following days. On the 22d, I obſerved, upon

the

the upper part of the arm, near the *deltoid* mufcle (which was ftill very large near the *biceps)* and the *extenfores cubiti*, two deep hollows ; and the extenfion of the elbow was made with great difficulty : wherefore I touched him chiefly upon thofe mufcles which cover the upper part of the arm, having increafed the electrical power in Mufchenbroek's manner; for he feemed to come on but flowly while the fhocks were but mild. But I made the neceffary difpofitions to hinder his being at the trouble of lifting up his arm to touch the tube ; I applied it to the hand and arm where I judged neceffary ; and, on the 23d following, he was able to lift and carry a weight of fixteen pounds and an half.

I repeated the operation on the 23d, 24th, and 25th. He was extremely fenfible of the flighteft fhocks on the laft day, fo as to excite compaffion in all that were prefent. Befides which, during the time that his arm was electrifying, I obferved certain protuberances in thofe places, from whence I drew the fparks, like thofe which profeffor Jallabert had feen in his patient ; but in thefe two days they became very large, and, upon rubbing the arm, the fkin peeled off : notwithftanding they diminifhed on the following days, altho' the fhocks were made more powerful ; which is the more remarkable. The deltoid mufcle, which, on the 22d, was obferved to be fo large, became much lefs ; and the hollows, which were between this and the biceps and extenfors of the elbow, were filled up ; but, upon bending the elbow, there remained ftill a preternatural rigidity.

On the 27th, the patient gave feveral proofs of the advantage he had gained by the electricity, in the pre-

4   fence

fence of feveral perfons: he opened and fhut the fingers of his right hand without the affiftance of the other: he could ftretch out and bend the *carpus* and *metacarpus* at pleafure : he took up from the ground his glove and other things, and a weight of $16\frac{1}{2}$ lb. above three feet, moving it backwards and forwards at the fame time ; and he could have employed more ftrength with the affected arm, than was fufficient to hold up the weight, without any inconvenience.

We then ftripped both arms, and found, that, as to the external appearance, the paralytic arm was become more like the other ; and the hand and fingers were better covered with flefh than before. Befides, feveral mufcles of the arm, efpecially thofe of the fore-arm, were fuller ; the blue and red colour of that hand difappeared, and it was now like the other. But the extenfion of the elbow was yet a little difficult ; nor were the fingers yet fufficiently flexible ; and therefore it was fomewhat troublefome for him to lay hold on, and keep, any thing in his hand. On the 28th, he performed all thofe proofs before fome profeffors and doctors of the academy with fuccefs. They viewed his arms and hands, and the change that was brought about was evident. I afterwards electrified his arm and hand ; but principally the extenfors of the elbow, the great extenfor, and the interoffeous mufcles, and alfo the *indicator* and mufcles of the thumb. The 29th he told me, that the arm fweated continually from the day before, which happened feveral times after being electrified ; and that the fweat would often continue till next day. On repeating, this day, my operation,

he

he fweated all over his body ; and having returned home, he felt as if a number of globules of blood flowed up his affected arm, w. ich made fo ftrong an impreffion upon him, that it frighted him ; but haveing ftripped his arm, no fort of alteration appeared.

On the 31ft of March, he was able to take up a glafs of beer in the paralytic hand, to hold it fteady, and put it to his mouth, drinking to the health of all the company one after another; and fince that time he helps himfelf at table with I is right hand. When he came to me on the firft of April, he lifted from the ground, almoft three feet, a weight of 33 lb. in the prefence of feveral perfons, which he was not able to do before, tho' he had tried feveral times. On this day the arm was electrified again in the parts that required ; but as the fenfation in this arm was nearly equal to that of the other, he was no longer able to undergo the fhocks, for an hour, as he ufed to do ; tho' he was very fenfible of the advantages gained by the electrical operation : however, he fainted away, and therefore we were forced to forbear a little. Befides this, he was often fubject to a loofenefs ; which Noguez at Geneva was alfo. On the 4th of April in the evening, I expofed the paralytic arm once more to the electrical fparks ; and as there always were fome perfons by at the operations, there was this time a Frenchman prefent, who had a megrim. He underwent the fhock twice, according to Mufchenbroek's method ; and came to thank me next day for having cured the diforder of his head. On the 5th of April, I again electrified my patient, and the fenfation was now nearly recovered in the paralytic arm, which was reftored to a healthy condition. He extended
his

his fingers, and contracted them at pleasure, could move the *carpus* and *metacarpus* at will; he took off his hat, and put it on, and had gained so much strength in his hand and arm, that he raised a forty pound weight to the height of three feet from the ground.

Such was the state of my patient after having electrified him fourteen times from the 10th of March, almost an hour each time. I did not touch his foot, which was a little paralytic from the beginning, having never been very troublesome to him.

From the 5th to the 27th of April, he was electrified eleven times more; during which time the strength of his arm still increased: he not only raised above 40 lb. weight with his right hand, moving it backwards and forwards at the same time, but he wrote his name, *Andrew Mauve*, with a craion, with the same hand, which he had not been capable of moving for fifteen years before.

John Godfrey Teske.

XXI. *An Account of some Observations relating to the Production of the* Terra Tripolitana, *or* Tripoli. *Humbly addressed to the Royal Society of* London, *by* Martin Hubner, *Fellow of the said Society, Professor of History in the University of* Copenhagen, *and Member of the Royal Academy of Inscriptions and Belles Lettres of* Paris. *Translated from the* French, *by* Emanuel Mendes da Costa, *F. R. S.*

Read June 21, 1759.
DURING a journey I made, in the autumn of the year 1755, through several provinces of France, principally in Britany, I made the natural history of that province, which has plenty of productions worthy the attention of a naturalist, one of the objects of my researches. The lead mines of Pouillaüen in the Lower, and those of Pontpean in the Upper Britany, employed me some time. The metallic veins in these mines are not only rich and regular, but also hold a great proportion of silver, which they do not extract with that profit, that might be done. In general, they could work these mines to greater advantage, if they were more skilful, or had proper machines, and above all others the fire engine, which is used with such success in the mines of Cornwall to drain off the water, that element being the greatest obstacle to the right working of the mines of Britany: but the advantages of this invention, glorious to its first discoverer, and useful

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to this nation, where it has been perfected, are yet fo unknown in France, that there are even mechanicians in that kingdom, who ferioufly doubt, whether the fire engine is any-wife ufeful. I fhall not here mention any thing of fome rare and curious foffils found in Britany, nor of the fquare ftones of a particular fpecies, on the formation of which the late M. de Robier, prefident of the parliament of Rennes, who had a magnificent collection of natural hiftory, employed his thoughts. I likewife fhall pafs over in filence the marbles and the plumb pudding ftones, called in France *cailloux de Rennes*, from the vaft quantities of them found in the neighbourhood of that city. The chief and only fubject I propofe in this differtation, is the generation or production of the terra Tripolitana, or Tripoli, of which there are great quantities, and of the beft kind, in Upper Britany. I more readily determine to give this illuftrious Society an account of my obfervations on this fubject, as not only the difcovery I think I have made thereon is very curious, but alfo that the generation of this earth has been hitherto utterly unknown, no one having, to my knowlege, explained, before me, in what manner it is produced. It is true, I heard in France, that a young gentleman, a native of Britany, had wrote fomewhat on this fubject, and that his differtation was to be inferted in a collection of mifcellaneous papers, or loofe pieces; but I never could fee his faid differtation, nor know what it contained; therefore I am incapable of judging what it is.

In the mountain de Poligné, called by the Bretons *le Tertre gris*, i. e. the grey hill, I think I have difcovered the true origin of Tripoli. This mountain,

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the Breton name whereof is manifeſtly derived from
the greyiſh colour of its ſummit, is ſituated in Upper
Britany, near the inn of Roudun, on the road from
Rennes to Nantes, a ſmall league wide of Baru, and
five leagues from Rennes. Obſervations and expe-
rience are the only means, that can conduct us to
certain knowlege in natural hiſtory; and it is conform-
able to that axiom, that I mean, ſimply, to lay be-
fore the Society what I obſerved in that mountain,
relating to the generation of Tripoli, and, at the ſame
time, to produce alſo the moſt authentic documents
of nature, in order to prove, that the terra Tripolitana,
or Tripoli, is probably only *a wood wholly petrified,
and afterwards calcined by the ſubterraneous fire.*

To give more weight to what I propoſe, and to
eſtabliſh this aſſertion, it will be neceſſary to obſerve
here, that the mountain of Poligne, in the interior of
which the Tripoli is found, has been, and is perhaps
yet, a volcano. Its colour, its form, its fiſſures, and
its ſtrata, prove it; and the inhabitants of the neigh-
bourhood declare, that they have formerly ſeen fire
on its ſummit at night; but that, however, for many
years paſt, they have not perceived any more. This
being granted, I have only now to give an account
of what is to be obſerved in the mountain itſelf, and
to produce ſpecimina of the different ſtrata of earth
found therein, to aſcertain the truth of my theſis.

The ſtratum of the true Tripoli, intirely calcined,
lies from 50 to 60 feet depth; the ſample of it here
produced is marked N° 1. This ſtratum is white; but
it ſometimes has a caſt of grey, and ſometimes is of
a reddiſh hue, as the ſample itſelf ſhews. It is quite
or fully calcined and converted into Tripoli, becauſe it
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lay near to the fubterranean fire of the volcano, fo as to be violently affected by it.

The ftratum above this, of which N° 2. is a fample, is already much burnt and calcined, but not enough to be whitened, having lain too far from the violence of the fubterraneous fire to be freed intirely from its heterogeneous and combuftible parts, infomuch that the fire has left it quite black.

The fmall layer or ftratum, which follows, that is, lies above it, or overlays the laft, and of which N° 3. is a fmall fample, is yet yellowifh, verging on brown; and altho' the burning is eafily feen on the extremities, and the effect of the fire in the interior part of the layer, yet it is eafily to be underftood, that it is the weaknefs of the fire incapable of reaching it with a force neceffary to calcine it, that has put it and left it in this ftate of imperfection. When one views this fample narrowly, veftiges, which appear pores of a wood, are eafily difcovered.

The next layer fhews the fame thing, but more clearly: the pieces marked N° 4. are famples of it. This layer has been lefs attacked by the fire than thofe under it; neverthelefs the lightnefs of the fubftance fhews, that it is already a little calcined, and the fight alone clearly demonftrates the refemblance of its pores to thofe of wood.

The piece taken from the fifth ftratum, and marked N° 5. confirms this fame obfervation. It is more weighty in proportion than all the others, becaufe it is lefs calcined; and indeed it is lefs calcined, only becaufe it was the fartheft diftant from the fubterranean fire, and thereby has fuffered lefs, as I have obferved before. However, its ends or extremities evidently

dently prove the action of the fire even on this higher or upper ſtratum ; and its interior parts likewiſe prove , as evidently, that this ſubſtance has been formerly petrified wood, but from which the ligneous parts have been driven away, and conſumed by a heat ſuperior to their reſiſtance.

I do not doubt, but had my time permitted me to make further reſearches, I ſhould have found in the mountain pieces of petrified wood not yet altered or deſtroyed by the action of the fire ; but my leiſure not anſwering to my wiſhes, I thought I might remain contented with what I have now the honour to preſent to the Society, and which ſeems to me ſufficient to determine the proceſs of nature in the generation or production of Tripoli.

If I am aſked how all this vaſt quantity of wood could be heaped or gathered together in this mountain ? I anſwer, 1°. That it is not to be ſuppoſed that the ſeveral layers or ſtrata of Tripoli, perfect or imperfect, follow each other without any interruption. 2dly, It may be allowed me to ſuppoſe a deluge, whatever it was, and which has covered many parts of our globe, to accumulate here all this wood, and even aid its petrification. 3dly, A further proof of my aſſertion is the wood-coals *, which inconteſtably are found deep in the earth ; for there are intire quarries of them in Saxony, in the neighbourhood of Halle, and which do not ſuppoſe a leſs quantity of wood : beſides, it is uſeleſs to diſpute againſt obſervation and experience, when they are ſolidly eſtabliſhed.

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* Wood-coals. The author uſes the words " charbons de bois ;" but what he means by his intire quarries of charbons de bois, I declare myſelf ignorant of.

I muſt

I muſt here further obſerve, that the volcano of Polignè did not, perhaps, ceaſe to flame on the upper part of the mountain, till after the ligneous parts of the wood buried in it ceaſed to furniſh the pabulum neceſſary to the fire.

I ſhall be happy, if theſe obſervations merit the approbation of this illuſtrious Society, to whom I have the honour to dedicate them ; but, however, am delighted, that they procure me an occaſion of teſtifying my zeal for this reſpectable and learned body, in communicating to it all obſervations, which appear to me uſeful.

However, I do not abſolutely pretend, that all Tripoli is a wood, wholly or *per totum* petrified, and afterwards calcined by the ſubterraneous fire. There are Tripolis of many kinds, as well as of ſeveral degrees of goodneſs ; and among theſe there may be ſuch, as are not otherwiſe than a real or native foſſil, deprived by fire of its primitive hardneſs and weight : but yet I believe, that the Tripoli, which has its origin from a petrified wood, muſt be the beſt, becauſe naturally it ought to be the fineſt, ſofteſt, and the beſt calcined.

London, 8th Octob. 1757.

Remarks on Mr. Hubner's *Paper on Tripoli.*

HE is of opinion, that the different ſtrata, marked in his ſpecimina N° 1, 2, 3, 4, and 5. are all the ſame, only under different degrees of calcination. But N° 2. ſeems to differ from the reſt, as it looks like mere charcoal, and appears to be very little, if

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at all, petrified or faturated with extraneous particles; I would therefore propofe, not only that this N° 2. fhould be further calcined, in order to be fatisfied, whether it will, by that means, come to be of the fame nature with N° 1. but likewife that the following N° 3, 4, and 5. which Mr. Hubner fuppofes to be lefs calcined, fhould undergo the fame trial, in order to obferve, whether they will by that means become Tripoli; alfo to know, whether by being calcined for fome time (before they are reduced to tripoli) they will put on the appearance of a coal like N° 2. for, if they do not, as I fufpect may be the cafe, it will be little lefs than a proof, that N° 2. which feems the plaineft wood of any, differs from the others more effentially than merely in its degree of calcination. Perhaps it may alfo be found, that N° 3, 4, and 5. differ not only from N° 2. but likewife from N° 1. and may not be capable of being reduced to Tripoli.

Remarks on the preceding Paper: In a Letter to the Right Honourable the Earl of Macclesfield, Pref. R. S. from Mr. Emanuel Mendes da Cofta, F. R. S.

My Lord,

YOUR Lordfhip's commands to wait on Mr. Profeffor Hubner, when in England, to receive his paper on the production of Tripoli, which he defigned for the Royal Society, and to difcourfe with him thereon, in order to tranflate it, and

lay it before this illuſtrious body, I accordingly
obeyed ; and the incloſed is the ſaid paper. My avo-
cations, which now abſorb many hours I formerly
dedicated to ſtudy, have been the only cauſe, my
Lord, that I have detained it ſo long; I therefore
hope for your Lordſhip's and the Society's pardon.

When I firſt undertook the tranſlation, I had
thoughts of giving my opinion thereon, not only by
reaſoning, but alſo by experiments. Time, my Lord,
has not permitted me to do ſo ; and ſince, having
ſeriouſly reflected, that Mr. Hubner, in his laſt para-
graph, turns his intire ſyſtem into a partial produc-
tion of one ſpecies of Tripoli, I think it unneceſſary to
trouble your Lordſhip, or the Society, with any ar-
guments pro or con. I ſhall only obſerve, that it is
not improbable but ſome of Mr. Hubner's Tripoli, as
he ſurmiſes, may have been produced from the petri-
fied wood he found in the mountain ; and the whole
account is then reduced to this only circumſtance,
that the layers of foſſil wood in this mountain, hav-
ing been ſaturated with the Tripoline particles, which
likewiſe abound in the ſame mountain, thereby com-
poſed a ſtone, or third body ; and that afterwards
theſe Tripoline particles were again reduced, by the
effects of a ſubterraneous fire, to their priſtine ſtate ;
the force of the fire deſtroying the compages of the
third body, or ſtone.

Had the wood, my Lord, been ſaturated with any
other metallic mineral, or earthy particles, I believe
every judge of ſcience will determine, that the calci-
nation of petrified wood, alone, could never have
changed it into Tripoli.

I have, in my hiſtory of foſſils, p. 76. 85. and 87.
deſcribed five kinds of the Tripoli earth ; and Mr.

Hubner's kind, here mentioned, is only a variety of that I call *creta, tripela alba dicta*, p. 76. N° 1. Your Lordship will pleafe to obferve, that of all the faid kinds none are produced in general by any fuch operations of nature, as Mr. Hubner intimates; therefore, my Lord, reafon will convince, that this is only a partial and local origin of his Tripoli, by concurring circumftances of wood and Tripoli buried together in the bowels of a volcano; for as we find this very fpecies elfewhere produced without fuch circumftances, it is certain they are not the fole efficient caufes of its production.

I am, with great fubmiffion and refpect,

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My Lord,

Your Lordship's moft devoted,

moft obliged, and moft obedient

humble fervant,

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Bearbinder Lane,   Emanuel Mendes da Cofta.
19 June, 1759.

XXII. *A remarkable Cafe of an Empyema. By Mr.* Jofeph Warner, *F. R. S. and Surgeon to* Guy's *Hofpital.*

Read June 28, MORRIS EVANS, aged 30 years,
1759.   on the 13th of March, 1759, was admitted into Guy's hofpital, with a remarkable complaint in his cheft, which attacked him in the month of Auguft 1758, with the fymptoms of a pleurify.
Upon

Upon infpection it appeared, that the left fide of the thorax was greatly inlarged, and prodigioufly diftended: the pectoral mufcle was fomewhat raifed up; on preffure it felt foft, and readily gave way: upon a removal of the preffure, the integuments refumed their former appearance, no marks of impreffion remaining on this, or any other part of the thorax, fo as to conftitute the characteriftic of an œdematous fwelling.

The fpaces betwixt the 9th and 10th, and betwixt the 10th and 11th ribs, counting from above, were vifibly inlarged, and fomewhat elevated: they felt foft, and yielded to the fingers; but were not at all inflamed, or otherwife difcoloured. Upon examination, I difcovered a fluctuation in both thefe parts. The general fymptoms that attended this cafe were fimilar to thofe arifing from all confiderable collections of fluids depofited in either cavities of the thorax: for inftance, the patient had a continual flow fever; a fhort cough, but without the leaft expectoration of matter; a great difficulty in refpiration, particularly in the acts of expiration. He was incapable of lying down on the right fide, without very great uneafinefs; he was much emaciated; and his countenance was uniformly fallow: he did not complain of fo much pain, or fo great a difficulty in breathing, when in an erect pofture, as I have fometimes obferved in difeafes of this kind even where the quantity of extravafated fluid has been much lefs; but at the fame time I muft acknowlege, that no fair inference could be deduced from hence, becaufe of the peculiar pofition the difeafed fide was put in when the poor man fat down, or ftood up;

either

either of which he was incapable of doing without
being fupported. The left fide of the thorax inclined
forwards, and protuberated in a peculiar manner, fo
as to give the head and trunk an horizontal pofture;
in which pofition of the body, the weight of the con-
tained fluid moft certainly was, in part, prevented
from preffing fo forcibly upon the left portion of the
diaphragm, the mediaftinum, and the right portion
of the lungs, as it muft neceffarily have done in a
more erect pofition of the body. He had one fymp-
tom, which I had never before obferved in patients
labouring under this complaint; that is, he was in-
capable of lying on his back, without bringing on
very alarming threats of fuffocation; but he did not
remember ever to have heard any noife or rattling of
the pus upon motion. He could lie moft conveni-
ently on his left fide; but even that pofture was of
late become very painful to him. In fhort, he could
find no tolerable pofture to put his body into, but
that of inclining it confiderably forwards, which (I
have already obferved) he was under a neceffity of
doing, to enable him to draw his breath; and I dare
venture to fay, that, upon attending to the fubfe-
quent part of the hiftory of this poor mortal's cafe,
the reafon, why fuch effects fhould be produced from
fuch a caufe, will very readily occur to thofe, who
have a moderate degree of knowlege of the formation
and ufes of thefe parts of the human body. Upon
making an incifion upon the moft prominent part of
the fpace betwixt the 10th and 11th rib, in the cavity
of the thorax of the left fide, at leaft eight Winchef-
ter quarts of a thin yellow matter, not at all foetid,
was difcharged upon the fpot in a full ftream: the
matter

matter iſſued thro' the wound by leaps, and was projected at the diſtance of two yards and upwards from the patient's body: he did not faint during the operation, nor afterwards; but from that moment he grew eaſy; his ſymptoms abated; he ſlept well at night; and the next day he had no bad ſymptoms, except that his difficulty in breathing was not quite removed. This, indeed, could not reaſonably be expected, conſidering the long diſuſe and extenſion which the diaphragm, the intercoſtal muſcles, and the reſt of the muſcles of reſpiration, had undergone from the preſſure and weight of the confined matter, joined to the injury which the lungs themſelves had ſuſtained. He continued with very little alteration till the fourth day, when his reſpiration became worſe, accompanied with bad nights, a quick pulſe, a great diſcharge of foetid matter from the wound, and a fluſhing in his face. Theſe ſymptoms remained till the 9th. To-day his breathing was better, his pulſe became more calm, the fluſhings in his face diſappeared; but the diſcharge was conſiderable in quantity, and he complained of a want of appetite, which, till now, had been tolerably good ever ſince the operation. On the tenth day, his difficulty of breathing returned; the diſcharge was very great, and offenſive; he had very little ſleep laſt night: in ſhort, he ſeems greatly upon the decline. 11th, 12th, and 13th, he continued much as on the 10th. 14th and 15th days he has had more ſleep than uſual; his appetite very bad; cough frequent, and troubleſome; diſcharge very thin, and conſiderable in quantity. Upon exerting his powers in the act of expiration, to diſcharge the matter from the cavity, there appeared

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an eminence betwixt the 7th and 8th ribs (counting from above) on the anterior lateral part of the thorax, which was very tender; but, upon his ceasing to exert himself, the swelling disappeared. 16th and 17th days, his cough very troublesome; the discharge from the wound was great; the matter very thin, and foetid; he gets very little sleep; his pulse quick, and low; his tongue dry, and parched: in short, to all appearance, he seemed to have a very short time to live. But, notwithstanding these threatening symptoms, he very unexpectedly survived till the 10th of June following (exactly 12 weeks from the time of his undergoing the first operation). In the middle of the month of April next preceding his death, he thought himself much better than he had been for many days before: his discharge at this time was much lessened; but his pulse was very weak, and fluttering.

About three weeks before the patient's death, a second opening was made by incision betwixt the 11th and 12th ribs by my collegue Mr. Way, under whose immediate care this poor man was from the day of his admission into the hospital, and who treated him with the greatest humanity and judgment. Three days before the patient's decease, the swelling betwixt the 7th and 8th ribs burst of itself, and discharged at least a pint of matter, that was exceedingly offensive.

Upon opening the body, the following particulars were discovered: the right portion of the lungs was greatly ulcerated on its superior part, where it adhered firmly to the pleura: on the inferior part of the cavity, the lungs adhered to the diaphragm; the

lobes

lobes of the lungs on this fide wère not fo much wafted as I expected they would have been, from the great degree of preffure which they had fo long fuftained from the prodigious quantity of matter, that was confined in the oppofite cavity of the thorax; nor was there any extravafation in the right cavity of the thorax.

The lobes of the lungs on the left fide were almoft intirely deftroyed: in this cavity there was near a quart of fœtid matter; the whole of its internal furface was ulcerated, and the two inferior ribs were carious in the neighbourhood of the fecond opening. In every other part the ribs were found; fo was the fternum.

The pericardium and heart appeared in their natural ftate. The injury done to the internal furface of the left cavity of the thorax was fo great, as to have deftroyed almoft the whole of the intercoftal mufcles on that fide of the trunk.

N. B. It may be worth remarking, that this patient did not any time fuffer the leaft inconvenience from the preffure of the external air, which entered into the cavity of the thorax thro' the incifions, as has been faid by authors to have happened in a great degree in the like cafes; but as that effect was not produced in this, or any other inftances of the like kind, which has come under my infpection, I am inclined to conjecture, that the bad effects of the preffure of the external air, when admitted thro' an opening made into the cavity of the thorax, is fuch an inconvenience as may rather be fuppofed to be likely to happen, than has been really known to happen often; the act of breathing having never been

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in the leaft fufpended, altho' the openings made into
the cavity have been very large, and the time taken
for dreffing the patient (fometimes once, fometimes
twice a day) has been very confiderable; but, on the
contrary, at every dreffing, I have obferved, that the
patient has breathed with greater freedom and eafe
than he could do before the performance of this ope-
ration, or even for fome hours before the dreffings
were renewed. As the matter in this difeafe lies
loofely in the cavity of the thorax, there is no greater
danger of wounding the lungs in this operation, than
there is of wounding the inteftines or omentum, in
tapping the abdomen in the afcites; and if the me-
thod be purfued, which I have recommended for
making the incifion thro' the intercoftal mufcles,
there will be no danger of wounding the intercoftal
artery, which is a circumftance much dreaded by
fome practitioners, but with no great reafon. How-
ever, was this to happen, it would probably not be
attended with any bad confequences, as the bleeding
might very readily be ftopped by preffure, or other-
wife.

Hatton Garden,   Jof. Warner.
June 28, 1759.

XXIII. _Ex-_

XXIII. *Extracts of some Letters from Signor Abbate* de Venuti, *F.R.S. to* J. Nixon, *A.M. and F. R. S. relating to several Antiquities lately discovered in* Italy.

LETTER I.

Rome, May 1st, 1757.

Dear Sir,

IN an inscription, which I found, while the front of the church of St. John Lateran was erecting, and which is now in the gardens of cardinal Corsini, without the Porta Aurelia (or St° Pancrazio) mention is made of the * *equites singulares,* as guards of the persons of the emperors.

Herculi Invicto Sacrum
Genio Num. Eq. Sing.
Augg. N. N. Pro Salute
Imp. Cesar. L. Septimii
Severi, et M. Aurelii An
tonini - - - - -
Et Juliae Aug. Matri
Castrorum. Aaug.

- - - - - Do
mus divinae. Trib.
Occo. Valente, et Octavio

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* This communication was occasioned by Dr. Ward's account of an inscription found at Malton (Philof. Tranf. Vol. XLIX. Part I. p. 69.) an extract of which had been transmitted to Signor de Venuti by Mr. N.

　　　Pifoni.

Pifoni. et. Ti. Exerc. Fi.
Titiano. et Aurel. Lupo
C. Julius Secundus
✝ . rexit. Ere. fuo. Deo. Do. D
Dedit. Idibus. Sept.
Severo III. et. Antonino Au
g g   N N. Cos.

As * Commodus was not afhamed to enter the
Lifts in the amphitheatre, as a gladiator (as appears
by an infcription, which I have lately publifhed) I do
not queftion, but that he might have a further ambi-
tion to be ranked among the *equites fingulares* alfo.

Without the gate of S^{to} Paolo, in the way to Oftia,
about 8 miles from Rome, there has been difcovered,
within thefe few days, a magnificent fepulchre of
very large dimenfions, and of a round figure. In the
middle of it was a fepulchral urn covered all over
with fculpture in no inelegant tafte. Among the
rubbifh, on the outfide, they met with this infcrip-
tion in large capitals,

‡ C. TUCCIUS. L. F. TRO. DVVOMVR.
     ACTIA.   UXOR.

Not far from this fepulchre, there are now found
æ es rufticæ, peafants houfes, adorned with rich
marbles in the walls, and with ftatues.

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† On the left-hand fide.
* This refers to Dr. Ward's Account, p. 79.
‡ *Note.* To this family (perhaps) might belong M. Tuccius,
who was ædilis curulis. Liv. I. xxxv. c. 41. Prætor. Ib. l. xxxvi.
c. 45. & triumvir. Ib. l. xxxix. c. 23.

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LETTER II.

Rome, Nov. 5. 1757.

DURING my fummer recefs at Viterbo, as I was tracing out the remains of antiquity in the adjacent country, I dropt, by mere accident, upon the ruins of Ferentum, a town of Etruria, different from that of the fame name in Latium, near Mons Albanus. Here, befides the walls of the city, confifting of wrought fqure ftone, I had the fatisfaction of finding a temple built of the fame materials, of neat workmanfhip, and a very elegant ftile of architecture: but what furprifed me more was a theatre almoft perfect, not only in the circular part of it, but alfo in that, which was taken up by the fcene or ftage. It had its porticos intire on the outfide, and likewife three entrances, anfwering to the *valvæ regiæ*, and the *hofpitalia*, defcribed by * Vitruvius: fo that nothing was wanting to render it complete, but the *orcheftra* and *pulpitum*. Thefe remains are acceffible to all the world ; yet no one hitherto has delineated or publifhed them. We have feveral valuable monuments in Latium, Sabina, Etruria, Campania,

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* The learned abbate refers here (I prefume) to Vitruv. de archit. l. v. c 6. (Cunei) qui funt in imo, et dirigunt fcalaria, erunt numero feptem ; reliqui quinque fcenæ defignabunt compofitionem, et unus medius contra fe *valvas* regias habere debet, et qui erunt dextrâ ac finiftrâ *hofpitalium* defignabunt compofitionem.

Ipfæ fcenæ fuas habeant rationes explicatas, ita uti mediæ valvæ ornatus habeant aulæ regiæ, dextrâ ac finiftrâ hofpitalia. Ib. c. 7.

For a fuller account of thefe entrances into the ancient theatres, vid. Montfauc. Antiq. explic. Tom. III. Par. II. l. ii. cap. 2, 3, & 4.

and

and Calabria, which contain subjects of the highest erudition, but yet yet are unknown to, and disregarded by, learned men ; while at the same time they are searching, with great expence and labour, after others in Greece and Asia, which are already known, and perhaps not so intire as these. I have caused a drawing to be made of the theatre above-mentioned, and some time or other (probably) may offer it to the public.

I have lately met with a curious dissertation, published by a professor of the university of Pisa, upon a gem, which exhibits the Theban war, with the names of five heroes engraved in Etruscan characters upon it. You (in all probability) saw it at Florence, in the cabinet of Baron Stosch, who a few days ago was struck with an apoplexy, and lies now at the point of death.

L E T T E R   III.

Rome, Jan. 31, 1759.

A FEW months ago, I published a dissertation upon a little marble relievo, inscribed with Greek characters of the smallest size. The subject of it is the story of Circe, as related by Homer, Odyss. lib. x. It is really remarkable, that whereas there are extant several ancient monuments alluding to the Iliad of that poet, very few are found, which refer to the Odyssey.

There has lately been discovered without the *Porta Prænestina* (or Maggiore) about four miles from Rome, an old sepulchral apartment, wherein were.

four

four farcophagus's, adorned with very curious re-
lievo's. Two of them were of a large fize ; the other
two of a fmaller. On the firft of the large ones was
elegantly carved the Fight of the Giants, who are
reprefented with thighs compofed of ferpents. The
fecond appeared to me to exprefs the Combat of the
Amazons and Thefeus. Of the two fmaller farco-
phagus's, one exhibited Bacchus in a chariot drawn
by centaurs, and preceded by the whole chorus of
the Bacchantes : the other feemed to be a battle (per-
haps) between the Greeks and the Trojans ; as one
part of the figures had long beards, tunics, and long
breeches : all of them were of exquifite workman-
fhip.

There has been lately dug up here an admirable
ftatue of Venus, with an elegant Cupid ftanding upon
a dolphin ; as alfo feveral curious infcriptions.

I have communicated your extract of Mr. Swin-
ton's moft learned conjectures concerning a coin of
Monefes *, a Parthian king, to Signor Corfini.   He
approved of the performance, and admired the judg-
ment and acute penetration of the author : but ex-
cepted a little to that part, where, in order to afcer-
tain the epoch of the piece, he [Mr. Swinton] ex-
preffes a doubt with regard to the *Victory* on the re-
verfe of it ; as there was no engagement that year be-
tween the Romans and the Parthians.   But when
Monefes had ufurped the throne, difpoffeffed the law-
ful heirs, and plundered the neighbouring nations,
it is no wonder, that he fhould ftamp a *Victory* on his
coin : which fymbol ought to be referred to fome

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* Philof. Transact. Vol. L. Part I. p. 175.

confiderable advantage gained over the Barbarians, and not over the Romans.

XXIV. *An Account of fome Experiments relating to the Prefervation of Seeds: In Two Letters to the Right Honourable the Earl of* Macclesfield, *Prefident of the Royal Society. From* John Ellis, *Efq; F. R. S.*

My Lord,   London, Jan. 18. 1759.

Read Jan. 18, 1759.
AS the fupplying our colonies with the feeds of ufeful plants, in order to have their produce imported from thence into England, inftead of the places of their natural growth in Europe, Afia, and Africa, as we do at prefent, is a matter of fome importance, therefore I am perfuaded, that experiments tending to promote fo ufeful and beneficial a work, will meet with the approbation of this honourable Society.

Among many ufeful feeds, which I fent governor Ellis in the year 1757, were fome acorns of the cork-tree, which were put in a box in fand. Thefe, he mentions in his laft letters, were intirely fpoiled in the voyage; and obferves, that the confined air in the hold of fhips, occafions fuch hot and penetrating fteams, efpecially in warm climates, that it difpofes all feeds, in common packages, to a fweating or putrefactive fermentation, by which the vegetative quality of many is intirely deftroyed: and therefore advifes,

vifes, that feeds fhould be fent in tight cafks, and placed on or near the deck, fo as to have the benefit of the frefh circulating air, at the fame time the tightnefs of the cafk would fecure them from the falt water.

In order to fend the governor a frefh fupply of cork acorns, in a growing ftate, I tried the following experiments on them to preferve them found; the effect of which I expect to have the honour to lay before this Society next fummer : but as I tried the very fame experiments, at the very fame time, on a parcel of frefh oak acorns, which I collected myfelf at Sydenham in Kent, the latter end of laft October, and have fince kept them by me in a box in a warm room, it may give us fome infight into what may be the fate of thofe that are fent abroad.

The experiments were made between the 25th and 30th of October 1758 ; and the acorns cut open to fee the effects, Jan. 17, 1759.

Experiment 1. Acorns of the Englifh oak fmeared over feveral times with a ftrong folution of gum arabic ; and alfo they had been dried in a window, folded in a piece of paper, and put into a deal box.

When thefe were cut open, they appeared hard, dry, and inclining to black, being quite perifhed.

When I firft thought of making this experiment, I imagined, that the perfpirable matter of the kernel of the acorns could not pafs through the glaffy, clofe fubftance of the gum arabic ; but experience has convinced me of the contrary.

Exp. 2. Some acorns, treated as in the firft experiment, were wrapped up in papers, foaked in a ftrong folution of gum arabic, each in a feparate paper :

3   after

after they had been dried, they were put in the box with the reft.

Thefe were fomewhat fofter than the firft, but decayed.

Exp. 3. Some of them were fmeared feveral times over with gum fenega ; and when they were dried in the window, and well hardened, were put in a paper into the deal box.

Thefe looked rather better than the two former parcels ; but unfit for vegetation.

Exp. 4. Some of the fame acorns were put into the middle of a cake of plaifterers ftiff loam, or fuch as the brewers ufe to ftop their beer barrels, and covered over near an inch on every fide. This foon became dry, without any cracks : it was about 2¼ inches thick ; and was placed with the reft, wrapped up in a paper, in the box.

The kernels of thefe were fhrivelled up, and grown quite dry and hard, like horn, the loam proving a ftrong abforbent.

Exp. 5. Some were rolled up feparately in thin flakes of bees-wax, warmed, to make it pliable, and put in paper in the box.

Thefe looked very well when they were cut afunder, and appeared likely to grow ; but were a little fhrunk.

Exp. 6. Some were rolled feparately in rofin, made pliable with warmth.

Thefe cut quite frefh.

Exp. 7. Some of them were rolled, each in a thin covering of a mixture of pitch, rofin, and bees-wax, called mummy by the gardiners.

Thefe cut as well, and looked as frefh, as if they had juft fallen from the tree.

The

The cork acorns, that were fent to Georgia, were inclofed in the fame fubftances with the foregoing, and put into a box filled with dry fand, quite full, and well faftened : this was put into a tight cafk, among papers and wearing apparel, and ftowed in the upper part of the hold of the fhip.

While I was making thefe experiments, I wrote to Dr. Linnæus, of Upfal, for his opinion of them, and for his method of preferving feeds in long voyages. I have lately received his anfwer; in which he confiders the great danger that attends feeds in warm voyages, in the fame light with governor Ellis, and has communicated to me a very probable method of preferving feeds in long voyages, which, he fays, has never failed. The following is an extract of his letter to me, dated the 8th of Dec. 1758, from Upfal.

" Seeds may be brought from abroad in a grow-
" ing ftate, if we attend to the following method :"
. " Put your feeds into a cylindrical glafs bottle, and
" fill up the interftices with dry fand, to prevent their
" lying too clofe together, and that they may per-
" fpire freely through the fand ; then cork the bottle,
" or tie a bladder over the mouth of it. Prepare a
" glafs veffel, fo much larger than that which con-
" tains the feeds, that, when it is fufpended in it,
" there may be a vacant fpace on all fides of about
" two inches diftance between both glaffes, for the
" following mixture ; four parts of nitre, and one-
" fifth part, of equal parts, of common falt, and fal
" ammoniac : thefe muft be well pounded, and
" mixed together, and the fpaces all round between
" the outward and inward glaffes well filled with
" it."

" This faline mafs, which fhould be rather moift,
" will always be fo cold, that the feeds in the inner
" glafs will never fuffer, during their voyage, from
" the heat of the air.

" This experiment has been tried, and has not
" failed."

<div align="center">

I am, my Lord,

Your Lordfhip's

Moft obedient, humble fervant,

John Ellis.

</div>

<div align="center">My Lord,</div>

IN a *letter*, which I took the liberty to addrefs to your Lordfhip, dated Jan. 18, 1759, relating to fome experiments, which I had made to *preferve the acorns* of *Englifh oaks* for a longer time than ufual, in a perfect ftate of vegetation, I there took notice, that I had fent fome acorns of the cork oak *to the governor of Georgia*, preferved in much the fame manner: but as the fubftances I made ufe of for this purpofe differed a little, I fhall *defcribe* thofe *experiments here more particularly*.

On the 27th of Nov. 1758, I prepared *feven parcels* of the acorns of the cork-bearing oak or ilex, in the following manner:

N° 1. 15 acorns, *each* covered over fingly with a ftiff folution of *gum arabic*, and afterwards rolled up in *gum'd paper*.

N° 2. 13 D°. *each* rolled up in a thin cover of common *yellow bees-wax*, foftened before the fire, and rolled up afterwards, feparately, in *white paper*.

<div align="right">N° 3.</div>

N° 3. 10 D°. *each* rolled up, as before, in *wax*, and afterwards each covered with a coat of *brewers loam moiftened* with a thick folution of *gum arabic.*

N° 4. 5 D°. *each* coated with *gum arabic*, and afterwards with *whiting moiftened* with a thick folution of *gum arabic.*

N° 5. 25 D°. *each* coated with *gum arabic*, and afterwards with *brewers loam moiftened* with a thick folution of *gum arabic.*

N° 6. 3 D°. *each* covered with *gardeners grafting mummy*, confifting of a mixture of beeswax, rofin, and pitch.

N° 7. 10 D°. *each* covered with *fullers earth* made into a *pafte* with a ftiff folution of *gum arabic.*

Thefe *feven parcels* were all put *into* chip boxes, filled with *dry houfe-fand*, and afterwards put into a *tight cafk*; and arrived in Georgia in *April* following. Governor *Ellis*, in his *letter* to me, dated from thence *May 6*, 1759. fays, of all thefe experiments, *none fucceeded but the parcel* N° 3. which had firft been covered with *bees-wax*, and afterwards with a pafte made of *loam and diffolved gum arabic.* We even find, that thofe that were covered with a thin coat of *bees-wax*, and afterwards *with paper*, did *not fucceed*; as their covering was not thick enough to *keep in their perfpiration.* This was the cafe with fome of the *Englifh oak acorns*, which I had coated in the *fame manner* in *October* 1758, and cut *open* in *November laft* 1759; their kernels being *fhrivell'd and decayed*: and thofe I had covered the fame time with a mixture of *rofin, bees-wax, and pitch,* tho' their

kernels

kernels were *plump* and juicy, yet they, by this time, were *turned brown and rancid*, by imbibing the *steams* arising from the pitch and rosin, and were rendered *unfit for vegetation*.

It may possibly be remarked, that it is no uncommon thing to receive the *acorns* of oaks *from* most of the provinces of *North America in a growing* state in *January*, and even in *February*; and therefore it may be asked, *why* it should require *more care* to send acorns of our growth *thither* ?

The reason of this appears to me, that as the summer heats of those provinces by much exceed ours; so consequently *their juices being higher maturated*, are not so liable to shrivel and decay as *ours* are; which, experience shews, are more *watery, and less oily* : tho', perhaps, if both kinds were packed up in a *dry, soapy earth*, and could be carried at a *cool season* of the year, I mean the winter months, they *might equally succeed*; but, in this kind of weather, we have *seldom an opportunity* to send them, so as to expect their arrival before the weather, in the southern parts of North America, begins to grow too warm, as the ships seldom arrive there till April.

The chesnut, next to the acorn, being the most difficult to preserve found during the course of one season, or a whole year, on the 23*d of February* last, 1759, I procured a parcel of *Spanish chesnuts*, just as they were imported, many of which were sounder than they generally are so late in the season : these I divided into *four parcels*, and put *each* parcel into a small *earthen jar*, involving them in the following substances :

Jar

Jar N° 1. 12 Chefnuts in mutton fuet.
   2. 12 D°. —— in bees-wax and mutton fuet,
      equal quantities.
   3. 12 D°. —— in bees-wax.
   4. 12 D°. —— in bees-wax and yellow rofin,
      equal quantities.

Thefe fubftances I *melted*; but did not pour them among the chefnuts, till I could *bear* my *finger* in them without the leaft fenfible uneafinefs, which I confidered as the proper teft not to affect the kernels by their heat, and immediately *immerfed the jar* to the brim in *cold water*.

As this experiment was made with a *view* to give thofe gentlemen fome hints, who go to the *Eaft Indies*, I placed thefe jars in a room, where they were *expofed* to the *unufual heats* of laft fummer; heat being the great promoter of the putrefactive fermentation of vegetables, and which it is very hard for fuch gentlemen to guard againft, efpecially as they are obliged *twice* in their voyage home to *pafs the equinoctial line*.

In order to examine the effects of thefe experiments, and to lay before the Society a fair account of them, I *broke* all the *jars on the 22d of November laft*, before fome ingenious gentlemen of the Society, very intelligent in thefe matters, and found, that jar N° 1. which contained the chefnuts immerfed in mutton *fuet*, proved *all rotten*, attended with a very difagreeable *putrid fmell*. Thofe in jar N° 2. were *moft* of them *found and frefh*, and their kernels as white and fweet-tafted, as when *frefh gathered*. Thefe were inclofed in half bees-wax, and half mutton fuet, melted together. Thofe in jar N° 3. were
           equally

equally found and well tafted, and had been inclofed in bees-wax only.

Though *part* of the chefnuts in thefe jars were *rotten*; yet it appeared plainly to be *owing to fome defect* in them when they were *firft* immerfed into thefe fubftances; moft probably to the latenefs of the feafon, when the experiments were made.

Thofe in jar N° 4. which were inclofed in half bees-wax and half yellow rofin, were *all turned foft and fpongy*, of a *brown* colour, and a moft *difagreeable tafte and fmell*, from the refinous fteams they had imbibed.

On the 24th *of November* laft, I *planted fix* of the chefnuts preferved in wax and fuet (N° 2.) and *fix* of thofe preferved in wax only (N° 3.) in two *garden pots*, and placed them in a very fpacious *confervatory*, belonging to my worthy friend Philip Carteret Webb, Efq; F. R. S. at his feat near Godalmin in Surry; where I have the pleafure to inform your Lordfhip and this honourable Society, that *many of them are already germinating*; which *proves* this *method* of preferving the larger feeds a very *proper* one to recommend to gentlemen that go to *China*, and other parts of the *Eaft Indies*, to preferve many kinds of valuable feeds in a ftate of vegetation during a voyage of a *whole year*, till they arrive here; and probably till they are carried to our fettlements in the American colonies.

It remains then, for gentlemen who go to the *Eaft Indies*, to place the feeds they preferve in *bees-wax*, *or bees-wax and fuet*, in the *cooleft part* of the *fhip*, to prevent thefe fubftances being affected with the heat of thofe parts, which far exceeds ours. *Perhaps Dr. Linnæus*'s method of inclofing them in a larger veffel,

veffel, and furrounding them with a *mixture of falts,* defcribed in my former letter, will *anfwer this end.* He fpeaks with fo much certainty of its fuccefs, that I think it worth the trial, efpecially when he *affures* us it *never fails.*

I am, my Lord,

Your Lordfhip's

moft obedient humble fervant,

London, Dec. 13. 1759. John Ellis.

P. S. Small feeds, in their pods, may be preferved by being placed thinly on pieces of paper, cotton or linen cloth, that have been dipt in wax, then rolled up tight, and well fecured from air by a further covering of wax.

XXV. *The Cafe of a very long Suppreffion of Urine.* By Ambrofe Dawfon, *M. D. Communicated by* William Heberden, *M. D. F. R. S.*

Read Nov. 8, 1759. R W. aged 23 years, tall and well-made, was feized, in the year 1755, with a weaknefs of one fide, which foon went off, leaving only one knee weak and fwelled; for which fhe was admitted into St. George's Hofpital.

On the 4th of April, 1756, fhe had a ftoppage of urine, and felt no difpofition to make any for two days. During the whole month of April, the difcharge of urine was very irregular, it having ceafed at one time for five days, and at another time for nine days. When the catheter was introduced, little or no urine was found in the bladder.

3 In

In order to relieve her, fhe had been directed to ufe both the warm and cold bath, bleeding, purging, turpentine clyfters, and a ftrong infufion of pareira brava, without any fuccefs : cantharides had like-wife been given her to the quantity of three grains in a dofe; and this dofe had been repeated four times in 24 hours; but without any other effect than that of occafioning pains in her throat, ftomach, and bowels, with vomiting and purging. A wet towel put round her waift, feemed to do the moft good, and brought away fome water two or three times; but afterwards ceafed to have this effect.

From the 26th of April, 1756, fhe made no wa-ter, and felt no want of making it, for many months; yet all this time fhe could eat once or twice a day, and was able to walk and ride. She was fparing in the ufe of liquids; fhe had but little fleep, no fweats, and her fkin had no urinous fmell : her breathing was often very laborious, with a dry cough : the ca-tamenia were irregular : there were oedematous fwell-ings in her limbs, abdomen, hips, and face; but, by the help of purges, and fpontaneous vomitings, which began in the third month of the fuppreffion, thefe fwellings were tolerably kept under. She vomited fometimes every day, and fometimes only every third or fourth day; and tho' thefe vomitings ufually came on prefently after dinner, yet what fhe vomited feem-ed to be mere urine, without any thing which fhe had eaten mixed with it. In the beginning of June 1757, the nipples of her breafts chopped, and dif-charged fometimes a watery humour, fometimes a thick matter, ftreaked with blood, and fometimes a humour approaching to the colour of urine : all thefe

dif-

difcharges had an urinous fcent, and feemed to leffen her urinous vomitings, which, from this time, became lefs frequent: but her legs, and efpecially her body, fwelled to an extraordinary fize, and fhe breathed with the utmoft difficulty.

On the firft and fecond of Auguft, 1757, after a total fuppreffion of urine for above a year and three months, fhe felt uncommon pricking pains, with great heat all down her back and loins, and about the belly and groin. On the fecond day, fhe voided about three ounces of thick, flimy matter, attended with fharp pains in the urinary paffages: this water was not high-coloured. The next day, fhe made water of a truly urinous kind with lefs pain; and continued to make a little water every day, the pain daily decreafing; and on the 7th fhe voided about a pint.

Since this, fhe has often had a fuppreffion of urine for ten or fourteen days; and it once lafted two months, during which fhe had no vomiting; but her body was very much fwelled. In July 1759, according to her own account, fhe did not ufually make above half a pint in 24 hours; and fometimes fcarce fo much in two days. The catamenia were then irregular; her fleep fhort, and difturbed; fhe had very little appetite; her legs fwelled; and the reft of her body wafted.

XXVI. *Several Accounts of the fiery Meteor,*
which appeared on Sunday *the* 26*th of*
November, 1758, *between Eight and*
Nine at Night; *collected by* John Pringle,
M. D. F. R. S.

Read Feb. 8, 1759. SINCE the paper concerning the me-
teor, of November 1758, was read at
the Society, I have been favoured with feveral more
obfervations relating to the fame appearance, which I
have connected with the former in fuch a manner,
as that we may now trace the courfe of that body,
from the fouth northwards, over a confiderable part
of this ifland ; and at the fame time form a better
judgment of its figure, height, magnitude, and velo-
city, than what could be drawn from the firft ac-
counts only. At prefent, I fhall lay before the So-
ciety all the inquiries I have made on this fubject, and,
in another paper, I fhall offer a few remarks, con-
taining the refult of thefe obfervations.

I. I faid at firft, that I had not heard, that this
body was obferved any-where fouth of London, ex-
cept at Silchefter ; but having fince read in one of the
Magazines, that it was feen at Plymouth, I wrote to
Mr. Mudge, Surgeon in that place, to be informed
about the truth of that article; who anfwered, "That
" he had made every inquiry in his power with re-
" gard to the meteor, and could find but two per-
" fons who pretended to have feen it, and thofe to-
" tally difqualified from giving him any fatisfactory
" account

" account, either as to its magnitude, direction, the
" angle with the horizon, or degree of light: that
" they only saw in general (after the body of the
" meteor had passed) a red glaring flash, which
" seemed rather to excite astonishment in them than
" curiosity. In short, that nothing more could be
" collected, than that it was, or might have been,
" seen at Plymouth."

Some time after, Mr. Mudge favoured me with
another letter; in which he says, " I think I can
" now venture positively to say the meteor was not
" seen at Plymouth. Besides a very minute and par-
" ticular scrutiny among the people of the town, as
" I was apprehensive the narrowness of the streets,
" and height of the houses, might have been the
" cause of their not observing it, the Lieutenant-
" governor was so obliging, at my request, to send a
" serjeant to inquire of every soldier in the garrison;
" and as some of them must have been on centinel
" duty that evening, I am thoroughly persuaded, if
" the meteor had appeared above their horizon, it
" could not have escaped them, as the garrison is
" situated on an eminence, and the prospect bounded
" by the sky only."

Dr. Huxham also acquainted me, that he did not
believe the meteor had been seen at Plymouth.

II. The reverend Dr. Shipley, minister of Silches-
ter in Hampshire, a parish about 45 miles * W.S.W.

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* Unless where it is otherwise expressed, I would be understood
to use all along the standard English measure of 69½ miles to a
degree.

of

of London), told me, " That he had not a view of
" the meteor himfelf, but had converfed with three
" countrymen, his parifhioners, who had feen it :
" that they had all agreed in obferving the light to
" be greater than that of moonfhine ; and that one
" of them, in particular, faid, it was fo great that he
" could eafily have feen a pin lying on the ground :
" that the body at firft was like a large fhooting ftar,
" but with a flower motion : that its direction was
" northerly : that, during its progrefs, it increafed in,
" fize, leaving a ftream of light behind ; and at laft,
" as it declined to the horizon, its lower part became,
" in appearance, as broad as his hand, whilft the
" length of the whole feemed to be about five feet,
" of a conical figure, ending in a point upwards :
" that, before it reached the horizon, it burft into a
" flame, refembling a flafh of lightning, and then
" immediately difappeared *." Dr. Shipley going to
the fpot where the obferver had ftood, and making
him point to fome trees at a diftance, over which, he
faid, the meteor difappeared, the Doctor found, by
taking the altitude with an inftrument, that it had

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* From this obfervation it appears, that the ftream of light,
called the tail, was not feen at firft ; probably becaufe the meteor
was in too high a region for the air to make any refiftance to the
flame; but when the body defcended lower, then the air, tho' ftill
extremely rarified, yet, from the extraordinary velocity of the me-
teor, would make fome oppofition to it, and drive the flame back-
wards to form the tail. That the meteor defcended obliquely, will
be more fully fhewn afterwards, tho' the circumftance mentioned
above *of a conical figure ending in a point upwards, as it declined
towards the horizon,* is one proof of that fact.

been

been extinguifhed about .° 15′ above the horizon, then bearing 35° weftward of the north †.

III. As for this city, and the parts adjacent, all I could learn was likewife from a Magazine; in which it was faid, " That the meteor was feen by three " gentlemen in Chelfea-fields." It is probable, that, on that evening, the air was foggy hereabouts, or that there was no wind to carry off the fmoak; for thefe circumftances will eafily enough account for there being no notice at all taken of that body in London, and that it was fo little heard of in the neighbourhood.

IV. Having heard it was feen at Colchefter in Effex, I defired the favour of Mr. Windham Bowyer, commiffioner of the excife, to employ fome of the officers of that diftrict to procure what intelligence they could about its appearance there. Accordingly Mr. Wigfon, collector at Colchefter, informed Mr. Bowyer, in anfwer to the queries fent him, " That " he had found a perfon who had feen the meteor, " on the 26th of November, about eight in the " evening; and who faid, that its direction, to the

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† This bearing carries the meteor about a point farther to the weftward than what is confiftent with the common maps, and feveral of the following obfervations.

Upon the fuppofition that the obferver was tolerably exact in pointing out the apparent altitude, at which the meteor difappeared, and that it was extinguifhed when nearly perpendicular to Fort William (in the Highlands of Scotland), as fhall be fhewn afterwards, then, allowing 22 miles for the curvature of the earth, and a diftance of 420 miles between Silchefter and Fort William, the real height of this body, at its difappearance, was about 32 miles.

" beft

" beft of his judgment, was about fouth-eaft; that
" the apparent diameter of the body was about five
" or fix inches, not fo large as the moon when at
" at the higheft, but more bright; that the meteor
" left a train of light behind it; that its progrefs was
" extremely fwift; that no explofion was heard
" when it difappeared; and that he did not perceive
" it to break into ftars in the manner of a rocket."

Finding this account difagreed fo much with
thofe I had received from other parts, with regard
to the courfe, I wrote to Mr. Wigfon, begging he
would once more fee the perfon, and defire him
to point out the path of the body, in order that I
might be fatisfied he had not been miftaken about its
motion to the fouth-eaft; and, in return to my let-
ter, that gentleman acquainted me, " That he had
" again converfed with the obferver, who ftill per-
" fifted in defcribing the courfe of the ball from the
" north-weft to the fouth-eaft; adding, that it ap-
" peared, at its greateft height, to have the fame alti-
" tude which the fun then had (March 12), at 10 in
" the morning *; but that it inclined to the horizon
" with great rapidity, and difappeared intirely, with-
" out difperfing, feeming to him to fall into a wood."
Mr. Wigfon concluded with obferving, " that as this
" man was at that time on a journey from Thorp
" to Colchefter, he might eafily be deceived as to the
" points of the compafs, by the windings of the
" road."

V. In tracing the progrefs of this body north-
wards, I was favoured with the following letter from

---

* Viz. about 32°.

Fig.I.

Fig.II.

M

R

T

Q

O

P

Fig.V.

Fig. III.

Fig IV.

A   B   S   C   E F   D   U   W

G   H

V

42° 20′   92° 30′

L K

N
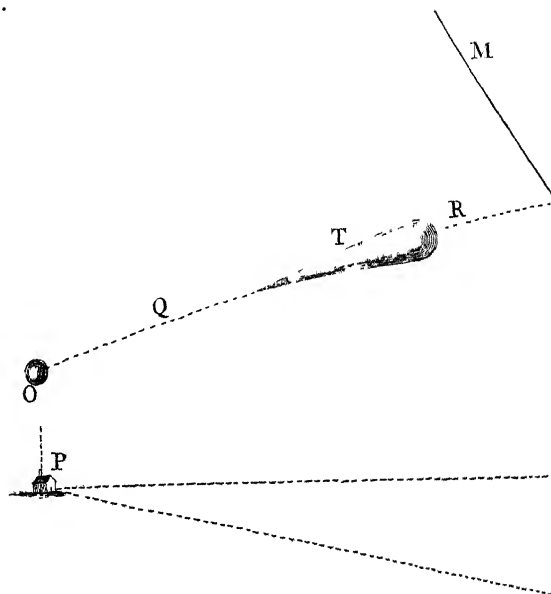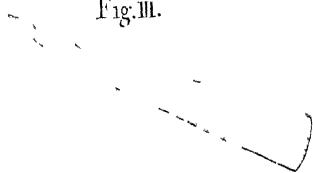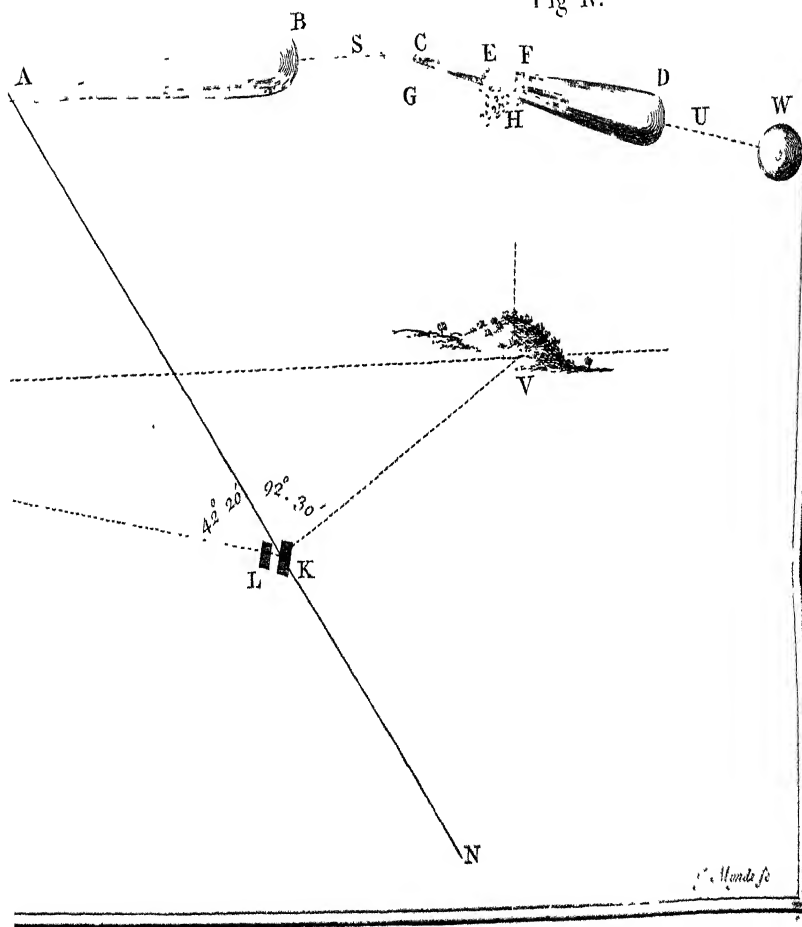
the reverend Mr. John Michell, fellow of Queen's College, Cambridge. " I promifed to fend you what " further account I could get from the perfon here, " a glazier by trade, who faw the meteor of the " 26th of November. I walked yefterday along " with him to the place where he was when he faw " it, taking with me a quadrant, to meafure fuch " altitudes as he was able to give me from his me- " mory, and a compafs to take the bearing. The " firft appearance, by the account he gave me, was " at leaft 70° high, and it appeared to move directly " perpendicularly *, till it came down to the horizon, " where it paffed between two trees, which he point- " ed out to me : this laft place of its appearance was " 23° weft of the north from the place where he " ftood ; and, as we were at leaft a mile diftant " from the trees, I believe you may depend upon that " bearing to a degree or two at moft. The whole " time of the appearance was (as near as my informer " can guefs), as long as he fhould be in walking near " 400 yards; but in this, I imagine, he is fomewhat " deceived, as I think I can collect, both from his " own account of it, and that of another perfon who " was with him, the time was much fhorter. The " head, which went foremoft (*Plate* VII. *Fig.* 1.) " was, by the defcription, of a bright white, like " iron, when almoft of a melting heat ; but it emit-

---

* In a fecond letter, Mr. Michell fays. " He afked the obferver " feveral times, whether the direction of the meteor did not vary " to the right or left ; and that he had affured him, that, accord- " ing to the beft of his judgment, it appeared to move exactly per- " pendicularly ; whence it muft have croffed the meridian in the " zenith, and moved in a great circle with regard to Cambridge."

" ted

" ted no fparks, as iron does in that ftate. The head
" was about half the diameter of the moon, and, till
" it had defcended to within about 14° of the hori-
" zon, was, as I apprehend by the account, fome-
" what lefs in the vertical than in the horizontal dia-
" meter; but, from 14° high, it was at its utmoft
" fplendor, and round, and continued fo till it difap-
" peared. The tail was about a fifth part of the
" breadth of the head *(Fig* 1.), and when the head
" was about 27° high, was at the longeft: the length
" then might be fomewhat more than 8°, which
" was the mean length. The colour of the tail was
" a dufkifh red, about the colour of red-hot iron,
" all of a breadth, not pointed. When the head was
" about 6° or 7° high *, the tail burft, as my
" informer expreffed it, and the brightnefs of the
" light dazzled his eyes; after which the tail difap-
" peared, and in the room of it there were three
" ftars, all contained within the compafs of a little
" more than one degree from the head *(Fig.* 2.); and
" they, together with the head, defcended, keeping
" their due diftance, till below the horizon †.
" The diameter of thefe ftars was nearly the fame
" with the diameter of the tail, viz. about 3'; but they
" were of the fame colour as the head. The
" brightnefs of the light was fo great, that (accord-

---

* Suppofing this angle of elevation juft, the real height of the
meteor, when the tail broke off, over the fhire of Lanerk in Scot-
land, was about 42 miles, allowing for the curvature of the earth.

† This circumftance of the head's falling under the horizon
might have been a deception; for at Silchefter, which was at a
greater diftance, the head difappeared before it came to the hori-
zon; but, perhaps, the horizon was clearer and lower there than
at Cambridge.

3

" ing

" ing to the account I received) one might fee to
" pick up a pin, and fome noife was heard; but of
" this latter, at leaft, I a little doubted, imagination
" being able to help out a good deal in fuch cafes.
" The appearance was about half an hour after eight
" in the evening, on a Sunday; but the day of the
" month, or the month itfelf, the man does not
" certainly remember. This is all the account I have
" been able to procure. The heights, &c. I could at
" firft only get in, *as long as from here to that hedge*;
" *as broad as a hat crown*; *as thick as my wrift*; *and*
" *about as long as a broom-ftaff*: but, as I thought thefe
" would give you no more ideas than they gave me, I
" defired my man to point out in the heavens, as near
" as he could guefs, clouds, &c. of the fame fize,
" and then I meafured them with a quadrant, and
" the refult of thefe meafures is what I have fent
" you."

VI. Purfuing the progrefs of the meteor north-
wards, my next information was from Manchefter.
Mr. Lloyd, of that place, Fellow of the Royal So-
ciety, wrote, that tho' it had been feen there by fe-
veral, the only tolerable account he could fend me
from that country, was from Lord Derby's head gar-
diner; who faid, " As he was returning from Liver-
" pool to Knowfley (a place at 7 or 8 miles diftance),
" about eight in the evening, he was furprifed by a
" fudden glare of light; and that he foon faw a ball
" of fire appearing, of half the breadth of the moon,
" moving horizontally eaftward *, a little inclined to

* This perfon, like the traveller between Thorp and Colchefter,
has probably been deceived in the direction by the windings of the
road.

" the north, with a hissing noise: that a train of light,
" like a tail, followed it, which being soon collected
" into the body, it burst, and part seemed to fall down
" like stars, and the rest vanished. He thought the
" whole appearance continued about two minutes."

VII. Mr. Lloyd added what follows from the
Liverpool News-paper, dated 1st of December, 1758.
" On Sunday last was seen in West Derby *, by se-
" veral credible persons, between 9 and 10 o'clock
" at night, a ball of fire, which arose in the east, and
" appeared to increase in size for some time, and then
" burst, without noise. Its direction was to the
" northward."

VIII. Cockermouth, in Cumberland, is about 86
miles north by west of Liverpool. Mr. Muncaster,
of that town, says, in a letter to a friend in London,
" That the meteor passed over that place † about
" nine in the evening, on November the 26th, with
" a very great velocity, towards the north-west; that
" it gave so strong a light, that the smallest thing
" might have been seen on the pavement; and that it
" disappeared in less than a minute: that the globe of
" fire appeared as large as the moon when she is high,
" but much brighter; and had a tail of a conical
" form; but that they did not observe any sparks or

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* The district so called of Liverpool.

† *Over that place.* This expression we are not to understand
here in the strictest sense, more than when used by the observer at
Carlisle: besides, as the meteor really passed within a few miles of
the vertical points of both those places, and probably was at that
time above 60 miles high, it could not appear to decline much from
the zenith.

" stars

" ftars fall from it, like thofe which are feen on the
" burfting of a rocket; nor did they hear any ex-
" plofion.'

IX. From Carlifle, which lies about 26 miles N. E.
of Cockermouth, the account I received was more
particular, and for which I was obliged to Mr. James
Hewit, wine-merchant of that city, who not only
had a view of the meteor during part of its courfe,
and heard a report, but, at my requeft, fome time
after, took the trouble to meafure the height, at its
apparent elevation when he faw it, and at its extinc-
tion, from the memory of another perfon, who had
a fight of it to the laft. I fhall give the fubftance of
feveral of his letters on this fubject, in his own words.
" The direction of the meteor was from fouth-eaft
" to north-weft. It did not appear in a globular
" form, as it paffed over Carlifle; but tapered in the
" manner reprefented in *Fig.* 3. Its head feemed
" to be about 14 inches in diameter, and its length,
" from head to tail, about 5 yards. It appeared much
" brighter than the moon, and lightned the atmo-
" fphere to fuch a degree, that a perfon, who ftood
" in the ftreet, could eafily have diftinguifhed the
" difference between a fmall needle and a pin, if
" they had been lying on the ground before him. It
" emitted feveral fparks as it went along, and con-
" tinued in fight about 25 feconds. About a minute
" after it difappeared, there were two explofions im-
" mediately following one another, of a hollow
" noife, as loud as the report of a cannon at 2 or 3
" miles diftance; and, immediately after the explo-
" fions, there was heard a confufed rumbling noife

" in

" in the air, which continued at leaſt 20 ſeconds; at
" the ſame time I could perceive no wind, and the
" ſtars were remarkably bright. As for the greateſt
" altitude, which you deſire to be informed about, as
" I could very well retain that in my head, a gentle-
" man in town, and myſelf, took that height with a
" theodolite, by pointing the inſtrument to that part
" of the heavens I ſaw the meteor in ; and in this I
" could be very exact, as I took particular notice of
" its apparent elevation above a certain high houſe
" it ſeemed to paſs over. We found the greateſt
" height to be 32 degrees above the horizon, on a
" vertical circle; and 41 degrees from the north to-
" wards the weſt *. As I did not myſelf ſee the me-
" teor extinguiſhed, by reaſon of ſome houſes that
" were in the way, I found a perſon in this neigh-
" bourhood, who had ſeen it to the laſt ; and getting
" him to point to that part of the heavens where it
" diſappeared, we alſo took that height with the
" theodolite, and found it eight degrees †: This
" man ſeeming to be pretty ſure of his pointing to the
" very place in queſtion, I believe you may depend
" on the obſervation as tolerably juſt. As for the
" figure *(Fig.* 3.), you have it as correct as I could

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* It appears by this obſervation, that the meteor being ſo low
as 32°, it muſt have paſſed the town a great way before Mr. Hewit
got a ſight of it.

† The meteor being extinguiſhed when perpendicular to Fort
William (as will appear by a ſubſequent obſervation), at the appa-
rent altitude of 8° at Carliſle, makes the real height at Fort Wil-
liam to have been between 26 and 27 miles, allowance being made
for the curvature of the earth, of 3½ miles, between theſe two
places.

" make

" make it ; only, perhaps, I have added too many
" sparks, as I doubt there did not so many issue from
" the tail. The meteor did not discharge any sparks
" nearer the thick end, than are described in the
" figure ; but those it did emit, darted from it with
" an astonishing velocity. In answer to what you
" require in your last (viz. whether the path of the
" meteor was to the eastward or westward of Car-
" lisle ?) my situation, when I saw it, was near the
" center of the town ; and the bearing of that part of
" the house, over which I saw it, was 41° from the
" north towards the west ; and as its progress ap-
" peared on the left side of me when facing the said
" house, the path was consequently west of me, and
" of any part of this city *."

X. Mr. Jonathan Ormiston, merchant at New-
castle, favoured me with all the information he could
procure in that part of the country. It consisted of
an abstract from the Newcastle Journal, and the copy
of a letter from Mr. Martin Doubleday, one of his
friends near Durham, whom he calls a gentleman of
sense and knowledge. The article from the Journal
is as follows. " Newcastle, 26th of November,
" 1758. This night a surprising large meteor was
" seen here, just about nine o'clock, which passed a
" little westward of the town, directly to the north,
" and illuminated the atmosphere to that degree for
" near a minute, that, tho' it was dark before, one

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* From this last circumstance, compared with observations V.
and XIII. we are enabled to judge nearly of the true path, which
must have run from Cambridge across the Solway frith, between
Carlisle and Dumfries, and by Obs. XVI. on to Fort William.

" might

" might have taken up a pin in the ſtreet. Its velo-
" city was almoſt inconceivably great; and it ſeemed
" near the ſize of a man's head.   It had a tail about
" two or three yards long ; and, as it paſſed, ſome
" ſay they ſaw ſparks of fire fall from it. It appeared
" low in the atmoſphere; and we are adviſed from
" Edinburgh, that it paſſed over that city * juſt about
" the ſame time, had the ſame appearance, and
" moved in the like direction."

Mr. Doubleday's letter.

XI. " On the 26th of November laſt, about a
" quarter before nine, as I was ſitting writing by
" candle-light, with my face towards a window
" fronting the north-weſt, I was ſurpriſed by a ſud-
" den and extraordinary light, and ſtepping haſtily
" to the window, ſaw the reſemblance of a large
" ſky-rocket, falling and burſting into ſparks of fire,
" which became more ſcattered in its deſcent, and
" ſeemed to be quite ſpent by that time it reached
" the horizon (which it did, as near as I could gueſs,
" due north ‡), its path appearing luminous to a
" conſiderable diſtance from the ſcattered parts,
" which with it were not diſperſed, but as if con-
" fined between two parallel ſtreight lines.   The
" greateſt height of its luminous path, when I firſt
" ſaw it, was 25 degrees above the horizon, N. W.
" by N.   I heard no noiſe at the time, nor after;

* By the accounts I had from Edinburgh, it was not nearly ver-
tical there.

‡ The head of the meteor, ſeen from this gentleman's houſe, could
not reach an unobſtructed horizon, nor be ſeen due north by 3 or
4 points, conſiſtently with moſt of the other obſervations.

" and

" and I conclude from its appearance, that it muſt
" have begun to burſt before I ſaw it."

Dated from Butterby, about a mile
   ſouth of Durham *.

XII. Mr. Blake, Fellow of the Royal Society,
ſent me the obſervation of the reverend Mr. Wil-
liam Henderſon, vicar of Felton (a village about
24 miles N. N. W. of Newcaſtle); who ſays, " That
" the night was dark and calm; that as he was go-
" ing home (a little after nine), about 20 yards weſt
" of the bridge, the road was inſtantly ſo much en-
" lightened, that he might have ſeen to take up a pin;
" that the globe, to the eye, was about the ſize of a
" ball of 6 or 7 pounds weight ‡; that he could not
" gueſs at its diſtance from the earth, but, during
" the ſhort time he ſaw it, he imagined he heard it
" whizz over his head †; that it had a tail like that
" of a comet, almoſt a yard in length, perpetually
" emitting ſparks of fire; that the time of obſervation
" was very ſhort, on account of a great hill that riſes
" on the ſouth ſide of the river Coquet, and of a
" clump of trees on the north ſide, which obſtructed
" his view; that its velocity was great, for that it
" did not continue in ſight above 5 or 6 ſeconds; and
" that its courſe, as near as he could judge, was to
" the north-weſt."

* Durham is about 59 miles from Carliſle eaſtward.
‡ The obſerver probably means a cannon ball of a ſix-pounder.
† This ſound muſt have been a deception, as ſhall be ſhewn
afterwards.

Theſe

These were all the accounts of the progress of the meteor, which I could procure in England: I shall next lay before the Society the observations I have had from Scotland; and as I have begun, I shall continue to trace the appearance from the south northwards.

XIII. Dr. Gilchrist, physician at Dumfries (which lies a few miles north of the Solway frith, and about 30 miles N. W. by W. of Carlisle), acquainted me, " That the best account he could get of the meteor " (which he did not see himself), was from a young " man of that place, who, in common affairs, was " sensible and distinct. This person, on the 26th of " November, 1758, in the evening, being in a room on " a first floor, which had the windows to the north- " east, was surprised by an extraordinary light, and, " running to one of the windows, saw a large fiery " body, like red-hot iron. It appeared to him as large, " and as long, as a middle-sized man, the fore part " broadest ; its progress was from S. E. to N. W. part " of the tail separated from the rest, but he still " thought it followed the body for a little space, and " then it burst like gunpowder, tho' without noise, and " fell down in sparks of fire, whilst the body kept on " its course * ; but which he immediately lost sight " of, by a house of two stories high that intervened." The doctor said further, that the same person formed his judgment of its height " from one of the steeples " of the town, a hundred feet high;" but, as the

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* The circumstances contained in this paragraph agree perfectly with the account of the farmer at Ancram. See Obs. XVI.

distance

diſtance of this ſteeple from the obſerver is not mentioned, nothing can be made of the apparent altitude from that deſcription, more than of the diſtance eaſtward, "which, letting fall a perpendicular from the "meteor, he imagined, was not above a good gunſhot "from him."

Dr. Gilchriſt added, "That a young lady of his "acquaintance, happening to be in the ſtreet near the "ſame place, ſaw the meteor likewiſe, and deſcribed "it as a ball of fire, about the bigneſs of the ſun, with "a tail; and the length of the whole as longer than "one's arm. She ſaid, it was almoſt over her head *, "higher than the ſteeple mentioned before; that it "burſt without noiſe, and was intirely diſſipated into "ſparks of fire, which fell down, and, as ſhe thought, "almoſt reached the tops of the houſes."

XIV. The reverend Mr. William Turnbull, miniſter of Abbotrule (a pariſh about 46 miles N. by E. of Carliſle, 44 miles N. E. by E. of Dumfries, and 6 miles S. W. by W. of Jedburgh, in the ſhire of Roxburgh), favoured me with a letter, containing the following particulars, "That on Sunday the 26th of "November, 1758, about nine at night, ſitting in his "parlour, which had a ſouth-weſt window, he very "diſtinctly ſaw a light, which he took for a flaſh of "lightning; but was ſurpriſed with the difference of "its colour, as being whiter, and giving a clearer

* From this circumſtance, and the greater apparent magnitude at Dumfries than elſewhere, it is probable the meteor was more nearly vertical there than at Carliſle, or any other place of obſervation mentioned.

" view of the pictures, and every thing else in the
" room, than what he could have expected from com-
" mon lightning ; that, however, he waited for a
" clap of thunder, and accordingly, at the end of five
" or fix minutes, he heard a very great explofion, not
" indeed fo like thunder, as the crafhing noife of the
" fall of a houfe; and being perfuaded that this was
" really the cafe, and that the gabel-end of his own
" houfe, fartheft from the room he fat in, with the
" offices, had fallen, he ran out, but found no damage
" done, nor faw any clouds, it being clear ftar-light."

XV. Having written to Mr. Walter Pringle, fhe-
riff-depute of the fhire of Roxburgh, for what infor-
mation he could give from that part of the country,
he acquainted me, "That he himfelf had neither feen
" the meteor, nor heard the explofion ; but that a
" fervant of the houfe, where he happened to be
" that night (about 20 miles S. S. E. of Edinburgh,
" and as far N. N. W. of Abbotrule), came in
" about nine, and told him there had been fome
" thunder and lightning ; which he thought very im-
" probable, as he had been out but a few minutes
" before, and had not feen a cloud in the fky'.

Mr. Pringle added, that the reverend Mr. John
Smith, of Jedburgh, had written to him as follows :
" I am furprifed, that, in all the accounts given of
" the meteor, feen on the twenty-fixth of Novem-
" ber, one remarkable circumftance is omitted,
" namely, the horrid crack, which I heard, being
" then on the confines of Cumberland, near Stone-
" garthfide (about 15 miles N. by E. of Carlifle): it
" was much louder than the report of any heavy
" cannon, and continued about 7 or 8 feconds. The
" people ,

"people thought it a peal of thunder; but this, I
"imagined, could not be the cafe, as the fky was not
"clouded. I did not fee the meteor myfelf, being
"then within doors."

XVI. In another letter, Mr. Pringle informed
me, " That he had converfed with one James Turn-
"bull, a farmer at Ancram, a village about three
"miles north-weft of Jedburgh, who had feen the
"meteor, and heard the exploſion, and who being a
"very fenfible man, he thought he could not give me
"greater fatisfaction, in anfwer to my queries, than
"by defiring him to write me a full account of what
"he had feen and heard." Accordingly I received
the following letter from that perfon.

"At Mr. Pringle's defire, I fend you an account
"of the meteor, as it appeared to me on Sunday the
"26th of November, 1758. My dwelling-houfe,
"at the mill of Ancram, fronts fouth-eaft; and the
"mill-houfe, which is directly before it, has the fame
"expofition, at the diftance of about twelve yards.
"About nine at night I happened to be out, and
"upon returning to my houfe, and juft entering the
"threfhold, the whole fide of the houfe became
"fuddenly enlightened, and with a brightnefs as
"of fun-fhine. My back being towards the place
"from whence the light came, I quickly turned
"about to fee what might be the caufe of it, and
"then beheld a globe of fire about the bignefs of the
"crown of the cap I commonly wear, 6 or 7, or at
"moft 8 inches in diameter *, directing its courfe

"from

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* After receiving this letter, I wrote to Mr. Smith, to defire him
to afk Mr. Turnbull, how many inches in diameter the full moon
appeared

" from a shepherd's house (which stood at above a
" mile's distance) towards me, as I thought, and right
" over the middle of the mill-house. I scarce had time
" to think, when it passed by me to the north-west
" with a very great swiftness, and very high in
" the air. When it came opposite to the gabel-
" end of the dwelling-house, I then discovered its
" true figure : it was perfectly round at the great
" end, which went foremost, and tapered three or
" four yards, to my imagination, in length †. Being
" resolved to see it as long as I could, and fearing
" the wall and roof of my house might intercept the
" view (for I was then standing in the threshold), I
" moved six or seven yards farther off the house,
" keeping my eye fixed upon the meteor, and ob-
" served, that it had not gone above a quarter of a
" mile, when one-third towards the small end broke
" off; which third separated into sparks of fire, resem-

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appeared to him when at the highest above the horizon; his
answer was, about 12 : hence the apparent diameter of the meteor,
to this observer, was not above one half, or at most two thirds, of
the apparent diameter of the full moon at its greatest height. But
whereas this person saw the meteor first, when it could not be
much farther advanced to the northward than Cambridge, the ap-
parent magnitude must have been considerably increased by the
time it came opposite to the gabel-end of his house, where it was not
at above the third part of the first distance. If therefore we allow,
that when seen at the largest at this station, the diameter of the head
was only equal to the apparent diameter of the moon at the same
height, we shall find the real diameter of the meteor to have been
about half a mile, upon computing its distance from the village of
Ancram.

† Having afterwards inquired, whether the head was of a glo-
bular form, distinct from the tail, he answered, " that he could be
" assured, that the appearance was as in the figure; and that the
" head and tail formed one tapering stream of light."

" bling

" bling ftars, and immediately vanifhed. Soon there-
" after the remaining body vanifhed alfo, directly to
" the north-weftward of my houfe, and the former
" darknefs returned *. At this time, I imagined I
" ftill faw the intire figure of the body in the air,
" tho' perfectly black ; but I have been fince told,
" that this appearance might have been only a de-
" ception, occafioned by the brightnefs of the body
" ftriking the eye ; as when we firft look at the fun,
" and then turn our fight to the ground, or a wall,

* I alfo inquired of the obferver, by Mr. Smith, whe-
ther the body went on for fome time in its progrefs northward,
after the feparation of part of its tail, or inftantly vanifhed ? The
anfwer was, " That the tail (meaning the part which broke off)
" went into fparks, and inftantly vanifhed ; that fome fparks came
" from the body alfo; but that it went forward a little way before
" it was extinguifhed ; viz. fo far as that he could be affured it bore
" then due north-weft of him." Upon this information, in order
to know the place of extinction, I drew on the map a line north-
weft from Ancram, till it interfected the line of direction of the
meteor, and found, that this point, by Elphinfton's map of Scot-
land, lay near Fort William ; by Moll's map of Great Britain,
the interfection was carried as far to the northward and weftward as
the weft end of the ifle of Skye: but as I relied moft on the former, I
have referred the extinction to a point perpendicularly above Fort
William. As for the feparation of the tail, that other remarkable
period, according to Mr. Pringle's meafures, it muft have happened
when the meteor was vertical to the fouthern and weftern part of
the fhire of Lanerk, near the borders of the fhire of Air, as was
obferved before.
  With regard to the apparent variation of the height, to the ob-
ferver at Ancram, Mr. Smith wrote as follows, " You inquire,
" whether, at the firft fight James Turnbull had of the meteor, it
" appeared to be as high as it did afterwards, when oppofite to his
" houfe ? His reply is, that, at firft view, it appeared indeed
" lower in the air than it did afterwards, which he afcribed to the
" greater diftance ; but that he cannot fay, that, at the end of its
" courfe, it was certainly lower than when he faw it firft."

                                                " we

" we fancy we ftill fee the figure of the fun, but of a
" dark colour. Upon going into my houfe, I looked
" at my watch, and found it five minutes after nine.
" The time of the meteor's appearance, during my
" obfervation, might be near a minute. After I had
" been in the houfe about five minutes, I heard a
" noife, like a clap of thunder, of fome continuance;
" and, upon my daughter's faying, there is thunder,
" I faid, that could not be ; for that I had feen no
" clouds when I was out. Upon this, I went out
" again, and found no clouds, but clear ftar-light.
" Several of my neighbours in the village of An-
" cram (which lies about 300 or 400 yards from
" me, a little wefterly of the fouth, and over the
" middle of which the meteor paffed, according to my
" imagination), likewife faw the meteor, and heard
" the report. One of them in particular fays, that
" the noife came from the fire as it went along; but
" I cannot be perfuaded of this, for, during the time
" of the light, I did not hear the leaft hiffing found,
" nor a noife of any kind. Another of my neigh-
" bours in that village, who heard the report, faid,
" it founded to him like a crafhing noife, and in fuch
" a manner as made him imagine, that the gabel-
" end of his own and his neighbour's houfe, which
" were contiguous, had fallen down at a time. I
" have been told, that the continuation of the noife,
" heard by our family, might have been owing to
" the particular fituation of the houfe. It ftands in
" a hollow, near the brink of the Ale, greatly over-
" topt by very high banks, partly of rock, partly of
" clay, and very fteep, which lie along that river.
" Upon the top of thefe banks, to the weftward of
" my

" my houfe, and farther up the ftream, is a large
" plantation, and it was directly over thofe trees that
' " the meteor difappeared. In this cafe, the found
" muft have come down fome part of the trough
" formed by the banks of the river; and it is be-
" lieved, that the continuation of it might have
" been occafioned by an eccho from the rocks, and
" the ruinous walls of an old monaftery, which
" ftands on the other fide of the water, juft oppofite
" to my houfe: whereas the village of Ancram lies
" off the trough of the river on a rifing ground, on
" a level with the plantation, over which the me-
" teor feemed to break. With refpect to the height
" of that body, all I can fay is, that I thought it
" very high, and can be pofitive it was nearer what
" Mr. Pringle calls the zenith than the horizon. I
" have pointed out the place to that gentleman, as it
" appeared to me; and, as he is to take the height
" of it with an inftrument, he will be able to inform
" you more exactly."

XVII. Before I received this letter, Mr. Pringle
had fent me the figures of the meteor, which James
Turnbull refers to. They were drawn by Mr. Smith,
in prefence of the obferver, and by his direction. *See*
Fig. 4.

    A B reprefents the meteor intire, after it had
    come fully in view. C D reprefents the fame,
    after the feparation. C E the third part of the
    tail feparated. D F the head, with the remain-
    ing part of the tail. G the fparks of fire iffuing
    from the part of the tail feparated. H the
                                   fparks

fparks emitted from the remaining tail, where the feparation was made. W the meteor reduced again to a round form, after lofing its tail.

Here it may be proper to obferve, that, with regard to the fpaiks, fome part of them are marked in the figure fomewhat different from what they are defcribed in James Turnbull's letter. There he only favs, that the third part of the tail, which was broken off, feparated into fparks of fire; without taking notice of the fparks that likewife iffued from the remaining part of the tail, which however was a circumftance he mentioned to Mr. Smith, when that gentleman drew the figure.

Some time after this letter, Mr. Pringle acquainted me, " That he had been at the farmer's houfe; that " he had furveyed all the places mentioned in his " letter; and, to make the whole perfectly clear, he " had drawn a plan, containing the apparent courfe " of the meteor, its apparent interfection with the me- " ridian, and the bearings of all the places mentioned " in the obferver's letter." *See the fame Figure.*

K the farmer's houfe. L the mill. M N the meridian line. P the fhepherd's houfe, over which the meteor firft appeared, making an angle M K P of 42° 20′ with the meridian. V the hill and trees, apparently over which the meteor broke, making an angle M K V of 92° 30′ with the meridian. The line P V the horizon. O the meteor feen firft in a round form over the fhepherd's houfe. T the meteor beginning to appear in its proper form. P Q R U the path of the tail broke off over the trees.

Thefe

These figures of the meteor are made much larger than they ought to be, in proportion to the other objects, in order the better to reprefent its true form.

After making this furvey, Mr. Pringle got the farmer to point to that part of the heavens to which he referred the meteor, when oppofite to the gabel-end of his houfe; and the obferver feeming to be well affured of the place, Mr. Pringle took the altitude with an inftrument, and found, after three trials, the height to be about 58° *. He concluded with faying, " That, in anfwer to fome more que-
" ries of mine, the farmer had told him, that he had
" obferved little rifing or falling of the meteor during
" its whole courfe; but that its motion, from the
" time he firft faw it, to its extinction, feemed to be
" nearly in one ftreight line, at an equal height above
" the horizon †; and that the light was continued
" and uniform, without any frefh burftings of flames
" from either the head or the tail."

XVIII. All the information I received from that part of the country, over which the meteor feemed to break, was from Lord Auchenleck, one of the judges in Scotland, whofe lands lie in the fhire of Air, bordering on the fhire of Lanerk. That gentleman was then at Edinburgh; but was fo ob-

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* At this time the meteor muft have been vertical, about two or three miles to the fouthward of Lochmabin, a town in the fhire of Dumfries diftant from the obferver about 37 miles, and from that place where the tail afterwards broke off 31 miles. From the altitude given here, I have computed the real height, at this place, to have been about 59 miles.

† This remark muft be corrected by the laft paragraph of the laft note of Obf. XVI.

liging, as, at my requeſt, to make all the inquiry
he could among the people on his eſtate, and his
neighbours; but could procure me no other ac-
count than what is contained in the following letter,
which he had from his gardener. "In obedience to
"your lordſhip's orders relating to that great meteor,
"which made its appearance upon the 26th of No-
"vember, 1758, as I did not ſee it myſelf, I cannot
"give ſuch a particular account of it as I could wiſh;
"but what I can collect from ſundry folks in this
"neighbourhood, is as follows. About a quarter
"after nine that night, there appeared from the
"ſouth-eaſt a very great illumination or light, which
"inſtantly made ſuch a ſplendor, that, to a conſider-
"able diſtance, one could moſt diſtinctly ſee houſes,
"trees, water, ſtones, &c. but could not obſerve
"any particular body from which the light iſſued,
"nor that it ran farther weſtward; from which we
"may conclude, that it had then broke. No noiſe
"was heard, and no ſuch thing as any aſhes were
"found, that I could hear of. During the preced-
"ing part of the day, we had a ſtrong and very
"cold ſouth-eaſt wind, with a little froſt; but the
"evening was more calm."

Signed, *James Bruce.*

From this letter it appears, that the ſky in thoſe
parts, as about London, was then ſo much clouded,
as to hide the body of the meteor, tho' the light of it
was very manifeſt, and which, I preſume, was the
brighter there for the burſting of the tail, and its
diſſolution into ſparks of fire, when almoſt vertical to
the obſervers.

XIX. Sir

XIX. Sir Robert Pringle, who was at Stitchill, (about 10 miles N. N. E. of Jedburgh, and about 60 miles, nearly in the fame direction, from Carlifle), wrote as follows. " I did not fee the meteor you mention,
" nor have I yet met with any body that obferved
" it, further than the great light with which it was
" attended, making every thing to be feen on the
" ground as diftinctly as in fun-fhine, and which
" continued, as they faid, much longer than a com-
" mon flafh of lightning from thunder. At that
" time I happened to be fitting, with fome of my
" family, in the parlour, and all of us heard a
" noife we could not account for, as founding
" like a gun fired off in the garrets, or a cannon
" difcharged about a quarter of a mile from us; but
" the noife continuing like thunder at a diftance, we
" concluded it was nothing elfe, till one of the maid-
" fervants came in, and told us, fhe had feen a very
" furprifing flafh of lightning, both for its clearnefs,
" which fhe compared to noon-day, and for its con-
" tinuance; but fhe did not hear the report, which,
" I fuppofe, was occafioned by the noife of her own
" feet: for all the reft of our fervants, that were fit-
" ting, and feveral other perfons in the neighbour-
" hood, heard it much as we did. Some of our
" Edinburgh news-papers defcribe the body of that
" meteor to have been like a large ftar coming from
" the fouthward, and ending in the northward, both
" points weftward of the obferver, with a train after
" it, in form like a cone; and with feveral fparks
" falling from it as it went along. Thefe accounts
" fay nothing of the length of this luminous appear-
" ance; but that it feemed to be about 10 or 12

" inches

" inches broad at the head; nor do they mention
" any found that was heard after it vanished. A
" gentleman from Berwick told me, that he had
" spoken with a master of a trading veffel there, who
" saw this meteor of November last, as he was fail-
" ing in the Baltick, and in the same form it ap-
" peared in this country; but did not mention its
" height, nor direction."

XX. Mr. Redpath, son of Mr. Redpath of An-
gelraw (a place about four miles north-east of Stit-
chill, in the shire of Berwick), says in a letter, dated
from his father's house, " That he did not see the
" meteor himself, nor had found any intelligent per-
" son who had seen it throughout its whole course;
" but that the best account he could procure was
" from one Mr. Mack, a farmer in the neighbour-
" hood, tho' he too had only observed it a little before
" its disappearance. That, from all he could gather,
" it was seen on Sunday the 26th of November,
" about nine at night; its direction was from the
" south-east to the north-west (but nearer the south
" and north points than the east and west), with a
" tail of a considerable length, pointing down-
" wards *, inclining to the east; that its course
" seemed to be very quick, and that sparks of fire
" fell from it as it moved along; that the whole was
" of a conical figure, and appeared to be about five
" inches at its basis; that a very strong light issued
" from it, which, in those houses where the candles

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* This circumstance of the tail pointing downwards, is con-
trary to the other observations, and must have been a deception.

" were

" were out, darted thro' the windows with such
" ftrength, that the rooms were wholly illuminated by
" it for 7 or 8 feconds; that its firft appearance was
" not exactly at the horizon, but a little above it *,
" and that, at its greateft height, it certainly did not
" exceed 40 degrees; that it was extinguifhed before
" it reached the horizon, perhaps by about 8 or 10
" degrees †; that the colour of the meteor was at
" firft nearly of a pure white, but, in proportion as
" it advanced, it grew red, and feemed to go out all
" at once; that the light, which iffued from it,
" feemed rather to confift of fucceffive flafhes from
" fide to fide, than of an uniform regular flame; that
" a few minutes after its difappearance (not above
" three or four) was heard by feveral people a violent
" thunder-clap, or fomething very like it, and from
" the fame point it difappeared, viz. rather nearer to
" the north than the north-weft."

XXI. At Dalkeith (a market town 6 miles fouth-
eaft of Edinburgh), a gentleman, who happened to
be walking eaftward in the ftreet, perceiving his right
fide and arm ftrongly illuminated, fuddenly turned
his face to the light, and faw the meteor, " then in

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* By Obf. XVI. the firft appearance muft probably have been
a good way above the horizon; but Mr. Redpath fays, he had
met with no perfon he could depend upon for the account of the
firft appearance.

† Angelraw being, within a few miles, of the fame diftance
from Fort William as Carlifle, we have, by this laft circumftance,
a confirmation of the real height of the meteor at its extinction,
upon comparing with this obfervation the notes of N° II. and
N° IX.

" a direction at right angles with the ſtreet ‡, having
" an altitude, as he conjectured, of about 45 degrees ;
" he obſerved that the figure was oval, the light great,
" and of a blueiſh caſt ; but he heard no ſound."

XXII. By an article publiſhed in the Edinburgh
news-papers, " the meteor appeared there of a conic
" form, about 5 or 6 inches broad at the baſis, and
" laſted 5 or 6 ſeconds ; its light was great, and
" ſparks flew from it like thoſe of a rocket, when its
" force is ſpent." I wrote to Dr. Whytt, Fellow of
the Royal Society, for more particulars from Edin-
burgh ; but he returned for anſwer, " That he had
" not ſeen the meteor himſelf, nor any body that
" had made proper obſervations upon it ; the reſt
" could only tell they ſaw a ball of fire, which emit-
" ting ſeveral ſparks in its progreſs, moved quickly
" along the ſouth part of the hemiſphere, and then
" diſappeared." He added, " that, on the night
" preceding that on which the meteor was ſeen, he
" had obſerved a very bright *aurora borealis*."

XXIII. One of my friends acquainted me from
Edinburgh, that the article in the Glaſgow paper was
to this purpoſe. " About nine o'clock laſt Sunday
" night (viz. on November the 26th), a globe of fire
" came over this city from the ſouthward, in ap-
" pearance as large as the full moon. It made the
" ſtreets as light as at noon-day, laſted about a mi-
" nute, and, juſt before it vaniſhed, it divided into

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‡ The ſtreet, to the beſt of my remembrance, lies nearly north-
north-eaſt and ſouth-ſouth-weſt.

3

" three parts directly over the middle of the town,
" and then ascended through the atmosphere *."
Mr. Smeaton, Fellow of the Royal Society, happened
to be that night at Glasgow; but did not see the
meteor, nor staid long enough to get any tolerable
account of it. From the information he had, he
judged it was not so vertical as the news-writer of
that place has represented it.

XXIV. From Dunfermline (a town in the shire of
Fife, about 14 miles north-west of Edinburgh), Dr.
Stedman acquainted me, " That he had only found
" two persons who had seen the meteor, a man and
" his wife, from whom he had the following parti-
" culars. That the figure was such as was delineated
" in the paper (*Fig. 5.*) which he sent me; that the
" first view they had of it was in the south-south-
" east, as it came from behind a building; and that
" it seemed to them to move westward; that the
" hinder part or train emitted large sparks or globules
" of flame, such as are seen to fall from a sky-rocket,
" when it begins to break; that its altitude was
" about 24°, which he had taken with an instru-
" ment, upon their shewing him how it seemed to
" move along the roof of a church, after bringing
" him to the window where they stood to see it;
" that they lost sight of it before it was extinguished,
" by a steeple that stood in the way; that its head

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* I desired a gentleman at Glasgow to ask the writer of the
paper, what he meant by this last expression; but he received no
satisfactory answer, and could furnish me with no better materials
from that place.

" or fore-part appeared fomewhat broader than the
" full moon; that no found was heard after its dif-
" appearance; that the time was about nine at night,
" on Sunday the 26th of November *."

XXV. In

* When Dr. Stedman fent me this account, he had omitted
taking the bearings; but, in his next letter, he told me, " he had
" fupplied that defect, and found, that the firft appearance to the
" obfervers (when the meteor came from behind the building that
" intercepted the fight of it), had been about fouth by eaft ¼ eaft;
" and that it had difappeared behind the fteeple at about fouth by
" weft ⅔ weft; that, during this fhort courfe, it neither feemed to
" them to afcend nor defcend."

By Moll's map, the firft of thefe bearings interfects the fuppofed
tract of the meteor in Weftmorland, about 14 miles weft of Ap-
pleby, diftant about 102 miles from Dunfermline; but cuts the
tract fo obliquely, that a very fmall error in the bearing, or in the
path of the meteor, would make a confiderable difference in the
following calculation. The other bearing cuts the tract near the
fouth-weft corner of the fhire of Lanerk, diftant about 46 miles from
Dunfermline.

The above meafures being in miles of 60 to a degree, give the
height of the meteor, when firft feen at Dunfermline, to be about
54 ftatute miles, and at the laft bearing to be about 24 ftatute
miles, upon making allowance for the curvature of the earth. I fuf-
pect, that the obfervers here made the apparent altitude too low;
but, however that may be, from hence the dipping or obliquity of
the courfe downwards is manifeft.

It was faid, that the head was fomewhat broader than the full
moon. Let us give fome allowance to the imagination, and fup-
pofe the head was but equal to the full moon, and that only when the
meteor was at the neareft. In this cafe, as the diftance of the moon
from the earth (about 240000 miles) is to the real diameter of the
moon (about 2180 miles); fo was the diftance of the meteor (about
59 miles) to its real diameter: which will thus be found to be about
half a mile. But if the apparent diameter of the head at its firft ap-
pearance (that is, at the greateft diftance from the obfervers), was
equal to that of the full moon, then the real diameter of the me-
teor was about ⅕ or ⅙ part more than a mile. If it was indeed
fomewhat larger than the moon, then the real diameter might have
been

XXV. In January after, I saw here one Mr. Cairns (a young man then appointed surgeon's mate to one of the regiments at Gibraltar), who told me, " That " he had seen the meteor, of the 26th of November, " about 9 o'clock at night; that he was then in the " shop of Mr. Oliphant, surgeon-apothecary at Cul- " rofs (a town about 19 miles W. N. W. of Edin- " burgh); that Mr. Oliphant and he were surprised " by a sudden glare of light from the street, coming, " as it were, in successive flashes, but without any " intervals of darkness; that they both ran out, and " observed a ball of fire moving, with great velocity, " in a direction nearly from the south-east to the " north-west; that its height seemed to be consider- " able; but that they had not seen it to the last, by " reason of some houses on the north side of the " street, which stood in their way; that it was some- " what of a less size than that of the full moon, when " about the same height above the horizon; and of " an oval figure, with the longest diameter in the " course of its direction. He observed no tail, nor " sparks of fire issuing from it; but said, that some " people of the town had taken notice of the latter; " that the meteor itself was of a reddish fiery colour, " though the reflection of the light from the streets " was of a yellowish cast; that he heard no explosion " himself, and had met with none who pretended to

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been about two miles; which is the length resulting from the obser-vation made at Dublin, as will afterwards appear. But, in such cases, the most moderate computations are most likely to be true; so natural is it for the imagination to magnify all objects that alarm it.

" have heard any noise, either during the appearance
" of the body, or after its extinction." Having in-
quired how long the light continued, Mr. Cairns an-
swered at first, " he believed about a minute or
" two;" but, upon looking at a watch, which had
a second-hand, and desiring him to recollect the time
from the first glare of light till the return of darkness,
he stopt me when I had counted 13 seconds *.

XXVI. I wrote to Dr. Simson, professor of me-
dicine in the university of St. Andrews (which lies
about 31 miles N. E. by N. of Edinburgh), who an-
swered, " That his family had been alarmed by
" the light, and that one of them cried out, the
" heavens were all on fire; that his son (a minister)

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* This account of the time is most likely to be the nearest to
the truth; since, without examining it in this manner, those, who
are unaccustomed to measure such small portions, will generally
reckon it much longer than it really is. I have been confirmed in
my opinion about the shortness of the time, by the observation of
another gentleman, who, being that evening at a friend's house in
the shire of Hadington, saw the light, without seeing the body. I
desired him to recollect the time, whilst I counted it, and in three
trials, he stopt me pretty exactly at 8 or 9 seconds. He imagined
he could not lose above 2 or 3 before he saw the light. Mr. Dut-
ton, watchmaker in Fleet-street, who, since this paper was first
presented to the Society, happened to see another meteor, which had
a course little shorter than the other, told me, that he could be
assured, it was over in about four or five seconds; tho' he believed
that others, not of his business, nor used to compute by small por-
tions of time, would readily have assigned a minute or two for the
duration of the appearance. But I shall suppose Mr. Cairns's mea-
sure, corrected by the watch, to be just; and therefore, as the
meteor, in 13 seconds, passed from Cambridge to Fort William,
a space of about 400 miles, it must have moved at the rate of about
30 miles in one second of time.

" hap-

" happened, at the firſt appearance of the light, to
" be ſtanding cloſe by a ſouth window, and ſaw the
" meteor like a ball of fire, but of an oval figure,
" with its longeſt axis in the direction of its courſe, of
" a ſize equal to that of the full moon at her greateſt
" height *; that it moved, with great velocity, from
" the ſouth-eaſt † (about which point he firſt ſaw
" it) towards the N. W. but that he had loſt ſight
" of it about the ·S. W. by the intervention of a
" building on the oppoſite ſide of the ſtreet, be-
" fore it had fallen from its apparent height: that
" he obſerved no tail, nor ſparks of fire iſſuing from
" it; and heard no noiſe after the return of darkneſs."
The doctor added, "That he himſelf was from home,

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* If Mr. Simſon loſt ſight of the meteor duly S. W. of him, it
muſt have then been perpendicularly over the ſouthern part of the
ſhire of Lanerk, about 66 miles from the obſerver, and about the
nigheſt he could have ſeen it any-where in its courſe. I ſhall there-
fore ſuppoſe, that it was at its greateſt apparent diameter juſt before
it diſappeared ; that is, equal to that of the full moon, according to
his compariſon ; conſequently its real diameter was about half a
mile, upon the like computation with that in the note upon Obſ.
XXIV. This is the moſt moderate; for the meteor might have
been conſiderably larger even from this obſervation.

† Having omitted deſiring Mr. Simſon to take the bearings
with a compaſs, he has not imagined that I required any greater
preciſion than having the moſt common points; but as I find
Cambridge laid down in all the maps nearly S. S. E. of St. An-
drews, and as we have no reaſon to believe the meteor was lighted
to the eaſtward of Cambridge, it is probable Mr. Simſon did not
ſee it till it was nearer to the ſouth than the S. S. E. But ſuppoſing
this gentleman ſaw it at its firſt ſetting out, viz. over Cambridge,
and duly S. S. E. then, from the angle of elevation of 15½ deg. the
diſtance between the two places, and an allowance made for the
curvature of the earth, the perpendicular height at Cambridge
muſt have been about 100 miles.

" at

" at fome miles diftance, at fupper, in a room with
" the windows darkened; fo that he neither faw the
" light, nor heard any found." In a fecond letter,.
he acquainted me, " That his fon, at my requeft,
" had gone with a friend to the fame window men-
" tioned above, and, pointing an inftrument to that
" part of the heavens he recollected to have feen the
" meteor in, they had found the apparent altitude,
" during the time Mr. Simfon faw it, to have been
" about 15 degrees and a half. As for the whole time
" of its continuance above the horizon, all the ac-
" count he could give was, that the body was not
" vifible to him longer than he could have counted 6
" or 7 ftrokes of his pulfe; but believes, if he had
" ftood at a higher and an open window, he might
" have feen it much longer."

One of my friends did me the favour to write to
Bamf and Invernefs, in order to learn what had been
obferved in the more northern parts of Scotland; but
found the meteor had not been taken notice of at
either of thofe places.

XXVII. Believing there was a better chance for
hearing of its courfe more to the weftward, a gentle-
man here was fo obliging as to procure me the follow-
ing letter, written by Dr. Alexander Mackenzie, phy-
fician in the fhire of Rofs, to a friend of his in the
fame country, on the occafion of this inquiry. " I am
" forry that the information I can give you about the
" meteor, of the 26th of November laft, will be fo
" little fatisfactory: however, I fhall tell you what I
" faw. I muft firft obferve, that where I then was,
" viz.

" viz. at Flowerdale (a gentleman's houſe on the
" weſtern coaſt of Rosſhire), the view of the heavens
" is extremely confined, being quite ſurrounded, ex-
" cept at one point, by very high and cloſe-approach-
" ing hills; whence you will underſtand, that the
" meteor muſt have been high before it could be ob-
" ſerved, and that it quickly diſappeared, as its pro-
" greſs was very rapid. Its light was moſt ſurpriſ-
" ingly ſplendent, but not in the leaſt like that of
" the ſun, except when it ſhines through a cloud,
" or a ſummer ſhower. Its magnitude was near to
" that of the full moon, when ſhe is three or four
" hours high. Its colour not at all like that of the
" body of the ſun, or an ignited globe, but reſem-
" bled that of the flame of ſpirits. Its figure was
" quite ſpherical, without any tail; but it emitted,
" or as it were dropped, ſparks of various colours
" and magnitudes. As for its height, it was vertical;
" and its direction was from the weſt northerly to
" the eaſt ſoutherly. I was ſenſible of no noiſe
" on its diſappearance. The time of night was
" about nine, and indeed as dark a night as ever I
" ſaw."

Upon reading this letter, and finding, by Dr. Mac-
kenzie's obſervation, the courſe of the meteor to have.
been ſo very different in thoſe parts from what I had
collected from the other accounts, and believing it
was beyond all chance, that a new meteor ſhould
appear at the ſame place, on the ſame day, and
at the ſame hour when the other was expected, I
doubted I had either miſunderſtood that gentleman's
expreſſion, with regard to the direction, or that he
poſſibly might have made a miſtake in the writing;
and

and therefore, to be clear in this circumſtance, I wrote to the doctor, deſiring to be informed, whether he meant to ſay, as I underſtood him, that the courſe of the body was *from ſome point a little northward of the weſt, to ſome point a little ſouthward of the eaſt*; or otherwiſe. To which letter Dr. Mackenzie obliged me withthis anſwer. " Altho' I regret my being out " of the way of anſwering your letter in courſe of " the poſt, yet, by that abſence, I have it now more " in my power to be exact with regard to ſome of " your queries, as I am juſt returned from Flower- " dale ; where, in conſequence of the firſt letter on " the ſubject of the meteor, I obſerved narrowly the " ſituation of the mountains over which it paſſed ; " in order that I might correct my account of its " courſe, if I had miſtaken it before. But, after " that ſurvey, I found my former deſcription ex- " act to a tittle, and your interpretation of my words, " viz. *from the weſt northerly to the eaſt ſoutherly*, to " be preciſely what I meant: they may not be terms " of art, but expreſs the true progreſs of that body " when I ſaw it; notwithſtanding that I obſerve, by " your informations from the ſouth of Scotland, and " from Carliſle, its direction was almoſt directly op- " poſite. What I meant by its vertical height was, " that its declination, if any, was extremely ſmall " from the zenith, but that northerly. Tho' I con- " tinued to gaze for about a quarter of an hour after " it diſappeared, I was ſenſible of no ſound, neither " like that of thunder nor a cannon : yet ſuch might " have been in a leſſer degree without my hearing it ; " as the noiſe of the ſea that night was remarkably " louder, than at any other time, during the whole
" mouth

" month I was at that place. I can pofitively affure
" you no tail was vifible there; tho', as I faid before,
" or at leaft meant to fay in my formei letter, the
" meteor emitted or dropt a great maɪy fparks of va-
" rious magnitudes, and moft beautiʃul colours, fome
" of which feemed to equal the fize of half a crown.
" My wife and another lady, at fifty miles diftance,
" almoft due eaft from Flowerdale, faw many fuch
" fparks, but no part of the body of the meteor. The
" light, tho' of the pale moon-colour where I was,
" yet was fo bright, that I could difcover every bufh
" and tree, every fcraggy rock on the tops of the
" mountains, altho' the night, both before and after
" its difappearance, was extremely dark, and with-
" out a ftar to be feen. I am not a little furprifed
" that, confidering how early in the night the meteor
" made its appearance, not one perfon, befides my-
" felf, in all this country, as far as I can learn (and
" I have taken pains to inquire), happened to fee it;
" except you will admit that my wife and her friend
" faw fome of its tract, from the fparks they ob-
" ferved. And this circumftance leads me to think,
" that it made a very quick turn immediately after
" its difappearance from my fight, lofing its fouth-
" ern direction, and running due eaft: which, in my
" opinion, confirms the fhip-mafter's report of fee-
" ing it in the Baltick."

Thus far Dr. Mackenzie, who, I am perfuaded,
could not be miftaken about the points of the com-
pafs, in a part of the country he is fo well acquainted
with, nor would offer any fuch account of a fact
he was not well affured of: fo that, upon the
whole, we muft refer this ftrange curvature in the
courfe

courſe of the meteor to ſome principle, at firſt view, very different from the common laws of motion ; but perhaps not altogether inconſiſtent with them, as I ſhall endeavour to ſhew in my next paper *.

I finiſhed my inquiries for the north, by writing to a friend in the iſle of Shetland ; but he had heard nothing of the meteor there.

XXVIII. I ſhall conclude with one account more, which I had from Dublin, in a letter from Mr. Cleghorn, author of the natural hiſtory of Minorca. I imagined, that a tolerable obſervation from that quarter would be uſeful, for aſcertaining both the height of this body above the earth, and its real magnitude; and accordingly I received from thence ſome proper materials for that purpoſe. Mr. Cleghorn writes, " That altho' the meteor, of the 26th of November, " did moſt certainly appear at Dublin, as well as in " England, yet few people had obſerved it with at " tention, and none, that he could hear of, had com " mitted any thing to writing, excepting one Mr.

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* It ſeems, from obſervations made on other meteors of this kind, that the curvature in the path of ſuch bodies is not extraordinary. Thus, one that appeared in Italy in 1719, recorded in the firſt volume of the Academy of ſciences at Bologna, did not purſue its courſe in a ſtreight line ; for they ſay, *directio non eadem ſemper fuit*. Again, in the hiſtory of the Royal Academy of ſciences at Paris, for the year 1738, we find another mentioned, which had ſo crooked a motion, that they call it *un mouvement bizarre*. Laſtly, I obſerve, that one of the gentlemen, who gave an account to the Royal Society of the meteor ſeen, about London, in the year 1741, deſcribes it as firſt ſhooting to the north-eaſt, and afterwards to the ſouth-eaſt. *See Philoſ. Tranſ.* N° 463. *p.* 59. *Abr. Vol.* VIII. *p.* 525.

" Garret'

" Garret, a good fenfible man, with fome mathema-
" tical learning, whofe account he had therefore fent
" me, in an extract from a regifter of the weather
" kept by that perfon; and to which was fubjoined
" an anfwer to fome queries, that had been put to
" him, concerning that body." Here follows the
paper.

> Extract from a regifter of the weather, by Tho-
> mas Garret, inn-keeper at Ifland-bridge, near
> Dublin.

" November the 26th, 1758, hard blowing wea-
" ther; wind at fouth-eaft. Fifteen minutes paft
" eight in the evening *, a globe of fire about feven-
" teen degrees above the horizon †, due eaft, moved
" from fouth to north, as large in appearance as the
" moon, but more of a golden colour; it broke and
" difperfed, like a ftarry rocket, in fmall, bright
" fparkles, nearly before the wind, or as if they
" paffed away with the wind.

* By the difference of longitude, this time anfwers to about
forty-one minutes paft eight at Cambridge, about half an hour
after eight at Carlifle, about 32 minutes after eight at Ancram,
about 26 minutes after eight at Edinburgh, and about 20 paft eight
at Flowerdale in Rofsfhire.

† By Moll's map, Dublin is diftant from Cambridge about 240
miles (at 60 to a degree); and a line drawn due eaft from Dublin,
cuts the tract of the meteor near the north-eaft corner of Derby-
fhire, diftant by the fame map about 185 miles from Dublin.
From thefe meafures, and the apparent height, the real altitude of
the meteor at Cambridge muft have been about 95 ftatute miles,
and over the corner of Derbyfhire about 72 ftatute miles, allow-
ance being made for the curvature of the earth. This obfervation,
compared with Obf. II. and XXIV. with the notes, makes the
obliquity of the courfe very manifeft.

" *N. B.* Mr. Garret keeps his clock very exact,
" by Glafgow's regulator, Chrift-Church.

" Emanuel Miller, of Ifland-bridge, faw this me-
" teor from beginning to ending ; thinks it was above
" the horizon about half a minute ; and fays, that it
" moved with lefs rapidity than falling-ftars com-
" monly do."

" The following queries were put to Mr. Garret,
" and his anfwers are annexed.

" 1. Was it a perfect fphere, or an oblong ball?

" *Anf.* A perfect fphere.

" 2. With or without a tail ?

" *Anf.* Without a tail.

" 3. Did any fparks of fire iffue from it?

" *Anf.* There did not any fparks iffue from it till
" it broke.

" 4. In what direction did it move, from S. E. to
" N. W. or otherwife ?

" *Anf.* It moved from S. towards the N *.

" 5. How long did it continue vifible ?

Anf. The obferver fays, he faw it only for the
" fpace of three feconds, or the twentieth part of a
" minute ; but that he is fure it had been vifible
" fome time before, he having been called out of his
" houfe by his fervant on purpofe to look at it.

" 6. Was any found heard like a clap of thunder,
" or the report of a cannon, after its extinction ; and
" how long after the difappearance of the light ?

* From that ftation, the declination to the weftward of the north
could not be well obferved.

" *Anf.* There was not any found heard: it was
" fuppofed to be at too great a diftance."

Thefe are all the accounts which I received, at
different times, upon this fubject. In another paper
I fhall offer to the Society fome remarks, that have
occurred to me, upon a careful review of the whole
materials.

XXVII. *Some Remarks upon the feveral Ac-
counts of the fiery Meteor (which appeared
on* Sunday *the* 26th *of* November, 1758),
and upon other fuch Bodies; *by* John Pringle,
M. D. F. R. S.

Read Dec. 20, 1759.
IN my laft paper, I laid before the So-
ciety all the accounts I had received of
the meteor, of the 26th of November, 1758 ; which
I could not reduce into a narrower compafs, without
the hazard of omitting fuch obfervations, as might be
judged neceffary for afcertaining fome of the principal
circumftances; or without paffing over thofe facts,
which, however immaterial they may appear at pre-
fent, might afterwards afford fome light in explaining
the nature of thefe *phænomena*. I have even inferted
fome particulars contradictory to others, that I imagined
more likely to be true, as I myfelf might be deceived,
and as, by preferving the feveral relations intire, I
thereby did moft juftice to the obfervers. The de-
ception of the fenfes, upon the appearance of unufual
objects, the fhort continuance of the meteors, and

the

the furprize occafioned by them, at a late hour, among people unaccuftomed to think on thefe fubjects, will fufficiently account, not only for the vaiiety, but the contradictions, in the feveral obfervations.

I come next to the refult of all the relations, with regard to the true path, the velocity, figure, magnitude, and other circumftances relating to this body ; which having deduced in the beft manner I could from the preceding materials, I fhall now fubmit to the judgment of the Society. But as my laft paper on this fubject was of fo great a length, I fhall not farther infift on it now, than barely to mention the conclufions I have made, without lofing time in recapitulating the feveral fteps that led me to them *.

Firft then, as to the path. This meteor feems to have been vertical at Cambridge, or nearly fo, and to have taken fire about the zenith of that place ; or at leaft to have appeared firft there in a ftate of ignition. From thence it proceeded directly, almoft N. W. by N. over feveral counties in England, over the Solway frith (which it croffed between Carlifle and the town of Dumfries) ; and in Scotland over the fhires of Dumfries and Lanerk : but foon after its becoming vertical to the laft, viz. a few miles to the fouthward of Douglas (or perhaps nearer to the borders of Lanerk and the fhire of Air, about 10 or 12 miles to the eaftward of Auchenleck), part of the tail feemed to break off, and to difperfe in bright fparks of fire ; whilft the head, into which the remainder of the tail was inftantly collected, moved on in the fame

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* Some of the moft material are fubjoined in notes to the former ' paper, fince it was firft prefented to the Society.

direction,

direction, till coming over Fort William, in the shire of Invernefs, after a courfe of about 400 miles, it there fuddenly difappeared.

But, notwithftanding the extinction of the meteor at this place, it feems ftill to have proceeded northwards ; fince it was feen again, in a luminous ftate, in a globular form, but without a tail, about the 58° of latitude, on the weftern coaft of the fhire of Rofs, almoft vertical to the obferver; moving then to the fouthward of the caft ; that is, in a direction almoft contrary to the firft : and in this laft courfe, of which we know not the end, it poffibly might have gone a great way to the eaftward.

During the firft part of its progrefs, viz. from Cambridge to Fort William, it went obliquely downwards, in fuch a manner, that, by computation, it muft have been from about 90 to 100 miles high at the firft of thefe places, and between 26 and 32 miles at the laft. But at what height it was afterwards feen in the fhire of Rofs, is not to be determined, fince one obfervation only was tranfmitted to me from that country. As Dr. Mackenzie obferved it nearly over his head, and yet of a fmaller fize than the full moon when fome hours above the horizon, I fhould fufpect, that, after its defcent to Fort William, it had reafcended at the time he faw it ; becaufe he probably would have defcribed it as a larger body, if then not higher than when it firft difappeared.

This dipping and rifing, in the courfe of a meteor, is not more extraordinary than its lateral deviation from a ftreight line, as I have elfewhere fhewn*. Such

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* See the note fubjoined to Obf. XXVII.

are the facts, tho', perhaps, it may be hard to account for them from the common principles of motion. What seemed moft likely to be the caufe of the vertical change of its direction, was its rapid defcent towards the earth, till it came fo low as that the refiftance of the air might act upon it; in which cafe, the lower part of the body meeting with denfer air than the upper, it would be fo reflected from that medium as to afcend; for tho', at the height of 20 or 30 miles, the air is extremely rarified, yet, in this inftance, the immenfe velocity of the impingent body will make a lefs refifting medium produce a greater effect. The meteor, therefore, might be reflected by the air, in the fame manner as a cannon ball by water, when it ftrikes it in a very oblique direction.

If this reafoning can be admitted, we may account for the lateral variation of its path, in the manner following. Although it fhould feem, that, during the firft part of the courfe, the wind was at S. E. yet, when the meteor advanced towards the north of Scotland, it there probably met with a wefterly wind; from this circumftance in Dr. Mackenzie's letter, *of the fea being that night remarkably louder than at any other time during a whole month he had been at Flowerdale:* for that place lies on the weftern ocean, in the fhire of Rofs; and there, I imagine, the fea could not produce that ftormy noife, unlefs the wind had blown from fome point between the weft and the north. If therefore the current of the air obliquely oppofed the motion of the meteor, that body would be gradually reflected into a new direction; from which, by another current, it might have been turned a fecond or a third time, until it fell at laft into that path

path defcribed by the doctor. From this account it will follow, that the lower a meteor defcends, the more irregular its courfe will be; as it will then meet with the moft refiftance, both from the denfity and the currents of the air.

In regard to the velocity, it feems almoft incredible; as we have fufficient *data* for computing it at the rate of 30 miles in a fecond. But if we allow, that it only moved through half the fpace in that time, we fhall find the progreffion of this body to have been above 100 times fwifter than the mean celerity of a cannon ball, and nearly equal to that of the earth in its orbit round the fun.

As to its real fize, we cannot pretend to determine that point with any precifion; fince its dazzling brightnefs would occafion fome deception, and the apparent magnitude has been fo differently reprefented by the obfervers. If the meteor, when neareft to Dublin, appeared to Mr. Garret equal to the full moon, then we fhall find, that its true diameter was about two miles; and if the farmer at Ancram faw this body, whilft it was vertical at Cambridge, of a fize equal to the crown of his cap, or to about half that of the full moon, we cannot allow lefs than a mile for the real axis. Upon eftimating from the obfervations made at St. Andrews and Dunfermline, the diameter was at leaft half a mile, and perhaps much greater. However, as the imagination is fo apt to enlarge fuch objects, we fhall put the fize of the globe at the fmalleft, and reckon it only about a mile and a half round.

The body muft furely have been of a confiderable bulk to have yielded fuch a light, as that, when in the

zenith

zenith of Cambridge, the farmer at Ancram (at the diftance of above 260 miles) fhould, upon entering his threfhold, fee the whole fide of his houfe illuminated by it; and, to ufe his own expreffion, *with a brightnefs as of fun-fhine*. And indeed the greatnefs of the light is every-where taken notice of, even at thofe places where the atmofphere was fo thick as to hide the tail, nay, the whole meteor, as at Auchenleck, where it was nearly vertical.

As for the tail, it was a ftream of light feveral miles in length; for this was no deception, like what we fuppofe the train of a fhooting ftar to be, but was either a real flame, or, what is more probable, it confifted partly of flame, but moftly of fmaller maffes of fire (which the obfervers call *fparks*, when falling out of the lucid tract), and of vapours or fuliginous particles, not heated red-hot, but illuminated by the parts actually burning. Perhaps thefe vapours were the chief part of the compofition, and which will account for its light being fo much fainter than that of the head; fince in fome places where the air was lefs clear, or the diftance greater, we find the whole meteor defcribed either as a round ball, or a fpheroid (with the largeft axis in the direction of its motion), but without a tail. In this laft cafe, viz. that of the oval form, it is probable that, befides the head, the beginning of the tail was alfo vifible, as confifting of flame, and therefore brighter than the reft; and that both together appeared oblong to thofe obfervers. But fuch as were neareft, and had a clear atmofphere, faw the tail of a confiderable length; that is, the flame, the fparks and the illuminated vapour, in a train behind the head, as being lighter,

3   and

and therefore meeting with more refiſtance from the air; in the ſame manner as the flame, the ſparks, and ſmoak, of a torch are ſeen to follow it. All this is plain; but, in regard to that ſeparation of the third part of the tail from the reſt, a circumſtance clearly deſcribed by the farmer at Ancram, and ſeemingly confirmed by other obſervations, there may be ſome difficulty. Perhaps at this period, upon a greater exploſion in the ball, moſt of the combuſtible matter was thrown out at once, which falling behind, occaſioned that appearance of the breaking off a part of the tail, whilſt, for want of fuel, the remainder vaniſhed, or, as the obſerver expreſſes it, *was collected into the head.* This account is rendered more probable, by what is ſaid of the emiſſion of a greater light about this time, and by the loud report heard by the farmer five minutes after, and·which, upon computing the diſtance, ought to have reached him much about that time, had it been occaſioned by this extraordinary burſting and diſperſion of the inflammable matter.

The hiſſing noiſe, taken notice of by ſome while the meteor paſſed them, was a deception of that kind, which frequently connects ſound with motion; and is the caſe of thoſe who fancy they hear ſomething, when they ſee the ſhootings of the *aurora borealis*; I ſay a deception, becauſe if the meteor, during its courſe, really made any noiſe, ſo great was the diſtance of that body, and ſo ſhort its continuance, that this ſound could not have been heard till ſome minutes after the return of darkneſs. But the final report, ſo frequently mentioned, not only heard by thoſe who ſaw the light, but by others who knew

M m nothing

nothing of what had happened, was a real found, and immenfely greater than any we are acquainted with. For, at the diftance of 70 miles and upwards, it was compared to loud thunder, the report of heavy artillery, the fall of the gabel-end of the houfe the perfon was in, and to a mufket fired off in the garret. If this noife was produced when the body threw out thofe maffes of burning matter (by the obfervers called fparks of fire, the burfting of the tail, and delineated in *Fig.* 5. as balls of a fmaller fize in the train itfelf), we fhall find, that at this time the meteor, by being more than 41 miles high, was in a region where the air is three thoufand times rarer than on the furface of the earth; that is, about fix times rarer than in a common exhaufted receiver, where fonorous bodies are not heard, and even where gunpowder and the *pulvis fulminans* take fire, and are exploded, but without noife. Hence I would infer, that the feparation of the elaftic matter muft have been performed with a velocity exceeding all imagination; as the intenfity of found fo much depends on the refiftance of the air, and as this elaftic matter could fly off with fo much celerity, as to find fo great an oppofition from fo thin a medium *.

I fhould alfo conclude from the great report, that the fubftance of the meteor was of a firmer texture than what could arife from mere exhalations, whether formed into a fphere, and then burning; or dif-

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* *M. Saluce* has lately fhewn, by fome curious experiments, that fuch fubftances as gunpowder and the *pulvis fulminans*, have a detonation in proportion to the rapidity with which the internal air is feparated, and to the refiftance of the external air. See *Mifcell. Taurinenf. Tom.* I.

pofed

pofed into a kind of train, and confumed by a running fire : for founds, as far as we know, are either produced by the quick and violent percuffions of hard bodies upon the air ; or by the fudden expanfion of an elaftic fluid, after being condenfed within fome folid fubftance. The noife occafioned by the motion of electrical matter is, perhaps, the only exception; but we have no reafon to imagine, that this was at all concerned in the prefent cafe. There feems to be the more ground for believing this body was folid, at leaft that its furface was fo, from finding, that, after the violent explofion, it ftill retained its form; a circumftance that could hardly take place, if the meteor had confifted of nothing but vapours. We may therefore prefume, that the burning matter found vent through a hard cruft by certain apertures, which either might have been there invifible, or unobferved. All I can fay in fupport of this conjecture is, that, by the Memoires of the Academy of Bologna, we find a meteor appeared in Italy in the year 1719, lower in the air than that we have now been treating of, and in which, it is pretended, four feveral chafms were diftinguifhed, each emitting fmoke *. To thefe arguments for the folidity of this body, we may add its extreme velocity, and the intenfity of the light; which are likewife circumftances more conformable to a heavy and folid fubftance, than to one formed of exhalations only.

Upon the whole, I believe it will appear, that thefe relations are not favourable to the prevailing

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* *Apparebant in eo hiatus feu voragines quatuor fumum exhalantes. Inftit. & Acad Bonon. Tom. I. p. 285.*

hypo-

hypothesis about the formation of such bodies, which makes them to consist of certain sulphureous vapours arising from the earth: for, besides what has been urged above, Dr. Halley has shewn, " That, at the " height of 41 miles, the air is so rarified as to take " up 3000 times the space it occupies on the surface " of the earth ; and that, at 53 miles high, it would " be expanded above 30000 times : but thinks it is " probable, that the utmost power of its spring cannot " exert itself to so great an extension ; and that no " part of the atmosphere reaches above 45 miles *." This being the case, how can we suppose any such vapours to rise to the height of 90 or 100 miles, where the air must be so many millions of times rarer than what we breathe ?   Again, we find that all vapours, in their ascent, mutually repel one other ; so little do they seem disposed to run together into globes of such a size.   But Dr. Halley observes (in treating of the great meteor of March 1719), " That " tho' the aqueous, and most of the other vapours, " are soon condensed by cold ; and wanting a certain " degree of specific gravity in the air to buoy them " up, ascend but to a small height ;" yet he adds, in favour of the opinion of these bodies being formed of exhalations, " That the inflammable sulphureous " vapours, by an innate levity, have a sort of *vis* " *centrifuga*, and not only have no need of the air " to support them, but, being agitated by heat, will " ascend in *vacuo Boileano*."   From hence he infers, " That the sulphureous vapours, disengaged from all " other particles, may rise far above the reputed

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* Phil. Transf. N° 181. p. 104.   Abridg. Vol. II. ch. 1. § 9.
" limits

" limits of the atmofphere; and contracting them-
" felves into a narrower compafs, by that principle
" of nature that congregates the *homogenea*, may lie
" like a train of gunpowder in the æther, till catch-
" ing fire by fome internal ferment, the flame may
" be communicated to its continued parts, and fo
" run on like a train fired," &c *. But this *hypo-
thefis* of that excellent naturalift feems liable to the
following difficulties. 1. What are the experiments
which fhew, that inflammable vapours have any fort
of a *vis centrifuga*; or will rife either in an abfolute
vacuum, or in a medium where the meteors are often
found, and where, according to the doctor, the air
muft be five hundred times rarer than in a common
exhaufted receiver †? 2. Altho' fulphureous vapours
may afcend from the earth, upon being agitated by
heat, will not that volatility foon ceafe, by the cold-
nefs of the air, long before they can arrive at the up-
per regions? 3. Does not all matter, capable of
taking flame, fuppofe a proportion of an acid falt,
adherent to the phlogiftic principle, and confequently
fome gravitation? 4. And will not the weight be
increafed by the large quantity of condenfed air, or
fome other elaftic fluid combined with the inflam-
mable vapours, which is the caufe of the explofion
and report? 5. Are we not led to this notion about
the innate levity and *vis centrifuga* of igneous matter,
from finding, that heat has a greater tendency upwards

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* Phil. Tranf. N° 360. p. 978. Abr. Vol. IV. Par. II. Ch. 1.
§ 28.

† His expreffion is, *above three hundred thoufand times rarer than
what we breathe.*

than downwards? but is not this owing to that portion of the air, which, receiving the heat, and being thereby dilated, becomes fpecifically lighter than the reft, and therefore rifes in the colder and more condenfed atmofphere? 6. Suppofe attraction to take place, fhould not thofe vapours, by that law, affume a globular form, and not, like a train, contract in breadth, and extend in length? 7. What is to fet this train on fire; fince a ferment, according to the chemifts, implies a mixture of heterogeneous parts, contrary to the doctor's *hypothefis?* 8. How comes this train to be drawn out in fo ftreight a line, and of fo equal a breadth, infomuch that, in the path of the late meteor, for about 400 miles, there was no fenfible deviation, nor any change in the magnitude of the luminous appearance, but what might be accounted for from its greater or lefs diftance from the obferver, or its being feen through a clearer or an obfcurer atmofphere?

Some have been of opinion, that thefe fiery meteors are only a kind of lightning, at greater heights than common; forming their notion upon the velocity of thofe balls of fire, and upon the found accompanying them, fo much refembling that of thunder. But this *hypothefis* having gained no credit, I need not employ time in refuting it; and the lefs now, as the nature of lightning is fo much better underftood than when this theory was firft publifhed. I fhall only obferve, that before the matter of lightning was difcovered to be of the electrical kind, it was natural to fuppofe it to be formed of the fulphureous vapours arifing from the earth; and if the earth was found proper for producing fuch exhalations, of

courfe

courfe it was judged capable of furnifhing materials for all the lucid *phænomena* in the æthereal regions. Thus, not an hundred years ago, the comets themfelves were accounted for on no better principle; and therefore we are the lefs to wonder if thefe meteors have been hitherto almoft conftantly referred, even by the beft naturalifts, to the fame origin.

Of all the *hypothefes* that have come to my knowlege on this fubject, a hint of Dr. Halley's (in a paper prefented to the Society feveral years before the above-mentioned), feems beft to agree with the late meteor; viz. that fuch bodies may be formed independent of any vapours from the earth *. But, with regard to the reft of his conjecture, that the one he defcribed, "might be a collection of matter formed in " the æther by fome fortuitous concourfe of atoms; " and that the earth had met with it newly formed, " and before it had conceived any great *impetus* of " defcent towards the fun," I fay, with regard to thefe circumftances, they are not at all fupported by the prefent cafe : for, if we confider the immenfe velocity with which the late meteor moved northwards (and in judging of which we could not be deceived by either the diurnal or annual motion of the earth), we muft be convinced that the earth did not meet with it before it had conceived a confiderable *impetus* of defcent, either towards the fun, or fome other attracting power. And altho' there was likewife a motion of this body towards the earth, as appeared by the dipping of its path, yet it did not come quite down,

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* Phil. Tranf. No 341. p. 159. Abr. Vol. IV. Par. II. Ch. 1. § 25.

as might have been expected, had gravitation to the earth been the only principle concerned; but it defcended no lower than to a certain depth in the atmofphere, after which it feems to have rifen again, and made a confiderable progrefs in the higher regions; contrary to Dr. Halley's opinion about fuch meteors, which, he believed, actually came to the ground *. And here I will venture to affirm, that, after perufing all the accounts I could find of thefe *phænomena*, I have met with no well-vouched inftance of fuch an event: nor is it to be imagined but that, confidering the frequency of fuch appearances, if thefe meteors had really fallen, there muft have been long ago fo ftrong evidence of the fact, as to leave no room to doubt of it at prefent. Their defcent, under the horizon, is fufficient to make the common obfervers believe they fee them come to the ground, whilft an explofion, high up in the air, coming late to their ears, paffes for the crafhing noife of the fall. Not that I call in queftion the poffibility of their touching the earth; which they are likely to do as often as they move perpendicularly towards it, and not in that oblique direction fo often mentioned, and by which means, it fhould feem, they are conftantly reflected by our atmofphere. All that I would conclude is, that hitherto we have had no certain proof of their fall; and it is to be hoped, that their motions, like thofe of the comets, have been fo regulated at firft by a governing Power, that we have nothing to apprehend from their aberration. Unlefs we fhould imagine, that the perpendicular defcent and burfting of

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* Vide loc. cit.

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one of thefe bodies has given rife to the ftory of Phaëton; fince no other event will, perhaps, fo well account for not only the foundation of that fable, but for a prevailing opinion among the ancients, that, befides a large tract of country, even the heavens themfelves, in appearance, had once fuffered by a conflagration.

If it is then probable, that thefe balls of fire come from regions far beyond the reach of our vapours; if they approach often fo near to the earth, and fo feldom or never touch it; if they are moved with fo much celerity, as in that refpect to have the character of celeftial bodies; if they are feen flying in all directions, and confequently have a motion of their own, independent of that of our globe; if they part with fuch quantities of an elaftic fluid, a phlogiftic matter, and probably an acid, furely we are not to confider them as indifferent to us, much lefs as fortuitous maffes, or trains of terreftrial exhalations in the æthereal regions; but rather as bodies of a nobler origin, poffibly revolving about fome center, formed and regulated by the Creator for wife and beneficent purpofes, even with regard to our atmofphere; which, during their combuftion, they may fupply with fome fubtile and falutary matter, or remove from it fuch parts as begin to be fuperfluous, or noxious to the inhabitants of the earth.

Since thefe fheets were printed off, Dr. Pringle received another letter from Sir Robert Pringle, acquainting him, he had not found that part of his intelligence hold good, of the fhip-mafter's feeing the meteor (of the 26th of November 1758) in the Baltick; but, upon further inquiry, had learnt, that though this perfon had obferved an appearance of the fame kind in thofe feas, it was not on the fame day.

Dr. Pringle thinks it likewise proper to inform the public, that, since his paper was printed, he has received two further accounts of the meteor, one from Mr Pringle, sheriff-depute of the shire of Roxburgh. the other from Mr. Garret, of Island-Bridge, near Dublin. Mr. Pringle says, "that having, since his last letter, drawn a more "accurate meridian, at the farmer's house at Ancram, he now finds, "that the shepherd's house (mentioned p. 236) bears nearly S. E. by "S. from the farmer's dwelling house; that the hill with the trees, 'over which the meteor seemed to break (laid down in *Fig.* IV. as at "92° 20' W. of the meridian), really bore W. by N. from the "observer; and that the bearing of the luminous body, at the point "of its extinction, was nearly W. N. W. and not due N. W. as the "farmer at first imagined, who, upon this last survey, was convinced "of his mistake." Upon this communication, Dr. Pringle drew, on two different maps, a W. N. W. line from Ancram to the path of the meteor, and found, that in Elphinston's map of Scotland, the intersection was a few miles to the eastward of Glasgow, but in Kitchin's map a little to the westward of it. In consequence of these last observations, the Doctor has fixed the point of extinction to the zenith of that city. But as Glasgow lies about 76 miles to the southward of Fort William, over which the meteor, in the preceding paper, was supposed to disappear, the first course of that body is reduced to about 324 miles in thirteen seconds of time, and its velocity therefore to about 25 miles in one second. Mr. Pringle has likewise acquainted the Doctor, "that the farmer, on seeing the engraving of the 4th "figure (of which Mr. Pringle had received a copy), had found fault "with the size of the head of the meteor, as being too large in pro-"portion to the length of the whole; since, to his imagination, the "diameter of the head did not exceed 8 inches, and that the tail seemed "to be about 3 or 4 yards long."

Mr. Garret acquaints Dr. Pringle, "that, since his first letter to the "Doctor, Emanuel Miller and he, having communicated their obser-"vations to Mr. William Gibson, mathematician, at Dublin, that "gentleman had come to Island-Bridge, and having made a survey "with his instruments, found, that the greatest altitude of the meteor "was no more than 12 degrees."

E R R A T A.

P. 240. *lin. penult.* for P Q R U, *the path of the tail broke off over the trees,* read Q R S U *the path.* E F *the tail broke off.*

In the plate, E F *the breaking of the tail,* should not have been represented as directly over the trees, but a little more to the southward, so as to bring the ball W (viz. the meteor at its extinction) perpendicularly over the said trees.

XXVIII. *Thoughts*

XXVIII. *Thoughts on the different Impregnation of Mineral Waters ; more particularly concerning the Existence of Sulphur in some of them, by* John Rutty, *Doctor of Physic.*

Read Nov. 15, 1759. INASMUCH as the existence of sulphur in waters hath been doubted, not only by Lister and Hoffman, but by another author, that has lately appeared, to whom the public has been, in some measure, indebted for exploding sulphur from some waters, on which it had been too liberally, and without the due evidence of experiment, attributed ; I have therefore thought it worth while to review, collect, and sum up, the evidences of sulphur in waters, in order to shew, not only that antiquity hath not altogether rashly attributed sulphur to waters, but how far the existence of that mineral is demonstrable to sense in several, and more especially the cold, waters of that denomination.

1. That the fetor of these waters is not owing to mere stagnation ; and that they possess something more than what common water acquires by putrefaction, appears not only from Dr. Short's observation of some of these having a full and brisk current, but because putrid rain-water, and many of our chalybeat waters, turned putrid by keeping, do not discolour metals, as these waters do.

2. The effects of these waters, and their vapours, in discolouring metals, and their peculiar smell and flavour, like that of boiled eggs, and in the stronger like that of rotten eggs, are perfectly similar to those

of

of the artificial folution of fulphur, and its va-
pours.

3. Many of thefe waters, both foreign and domef-
tic, are found to contain the native alcaline falt, which
is the proper menftruum for, and has the fame effect
in, diffolving fulphur, as the artificial alcali; viz. not
only the hot waters of Aix la Chapelle and Borfel, but
the cold of Geronfterre; and, in England, thofe of
Chadlington, Nottington, Bilton, Quincamel, Sutton-
bog, and Wigglefworth; and the following in Ire-
land, viz. thofe of Swadlingbar, Derrylefter, Lif-
blenk, Afhwood, Derryherce, Anaduff, Aghaloo,
and one lately difcovered at Lucan near Dublin.

4. Accordingly a milkinefs, or incipient precipita-
tion, analogous to lac fulphuris, is produced in feve-
ral of thefe waters, by dropping acids into them; par-
ticularly in thofe of Aix la Chapelle, even according to
Dr. Lucas's own teftimony of the effects of diftilled
vinegar on it, and in that of Moffat in Scotland; of
Harrigate; in our Swadlingbar water, and another of
our fprings of this fort in the C. Fermanagh : and
to this add the white hairy mucus ordinarily precipi-
tated on the fticks, or grafs, in the paffage of thefe
waters, analogous to a magiftery of fulphur.

5. The fulphur in waters is in a moft highly atte-
nuated, fubtile, and fugitive, ftate; infomuch that, as
Dr. Lucas obferves of thofe of Aix la Chapelle, there
is a great alteration in the colour of the precipitate
caufed by folution of filver in that, which hath been
immediately drawn from the fource, and that which
has lain by only twenty feconds, even in a bottle
filled, and clofe ftopt; fo foon is it loft or diffipated :
and moreover, it is alfo blended with other minerals;

fo

fo that it is no wonder it fhould be difficult to exhibit
a palpable fulphur, and that the diftinguifhing appear-
ances proper to that mineral fhould often fail, parti-
cularly the above-mentioned teft by acids, and the
burning blue.

6. That a real fulphur, or bituminofo-fulphureous
fubftance, is diffolved in thefe waters, and fubfifts in
fome of their lefs volatile or more fixed parts, is evi-
dent from the following appearances in the mud and
fcum collected from feveral of them : for the mud of
feveral of the cold waters I have called fulphureous,
as well as that of feveral hot baths in Germany and
Hungary, mentioned by Browne in his Travels, is
variegated with the feveral colours of yellow, green,
and red, as the real fulphur, and, in fome experi-
ments, burnt with a blue flame, and a fulphure-
ous fmell; and the like evidences may be given of
the fulphureous quality of the fcum of divers of our
cold waters, particularly in that of Mechan, in the
north of Ireland, which, being dried, exhibited on
the upper fide a whitifh yellow, or cream-colour;
but underneath a deep grafs-green, a pale, beautiful
gold-colour, and a light reddifh pink-colour, inter-
fperfed in a fubftance of a leaden blackifh colour;
every colour excellent in its kind, and as flippery as
frogs fpawn: varieties of colours, like thefe, being
alfo found in the preparation of lac fulphuris vari-
oufly expofed to the air. But, to come to more direct
proofs, we are affured, in Short's firft volume of his
Hiftory of mineral waters, as to the cold waters of
Harrigate, that both the mud and fcum burnt with a
blue flame, and fmelt ftrong of fulphur; and that
great quantities of yellow fublimed flowers of fulphur
have

have been found under the bafons of that well. And
Dr. Peter Shaw affirms, that real brimftone, even
found to be fo by proper trials, hath been feen float-
ing in the water like feathers, and feparable by bare
ftraining: and to all this agrees the obfervation I have
frequently made on feveral of thefe cold waters, viz.
certain light purple-coloured pellicles are frequently
found floating in them, which, being dried, fparkle,
flame, and ftink, on the red-hot iron. But, to con-
clude, the operation and effects of feveral of thefe
our cold waters, altogether fimilar to thofe of ful-
phur, abundantly confirm their impregnation with
that mineral.

Thus it appears, that fulphur is not confined to the
hot baths of Aix la Chapelle, and a few more abroad,
but is found alfo in the cold waters of both England
and Ireland; and as thefe have, of late years, been
fubjected to a minute examination, I fhall fubjoin a
brief comparifon between the one and the other from
experiment and obfervation : thus,

1. Is the fmell of the waters of Aix la Chapelle
like that of the wafhings of a foul gun, or like that
of the folution of fulphur in an alcaline lye? So is
that of our cold waters called fulphureous.

2. Do Aix la Chapelle waters, taken from their
fource, turn filver of a gold colour, and blackifh; and
with its folution, and that of fugar of lead, exhibit
a dark-coloured precipitation? So do our waters
called fulphureous.

3. Does Aix la Chapelle water, on dropping di-
ftilled vinegar into it, exhibit a milkinefs, analogous
to lac fulphuris? So divers of the cold waters above-
mentioned do alfo exhibit a white cloud with other
acids.

<div align="right">4. Do</div>

4. Do the waters of Aix la Chapelle and Borſel contain an alcaline ſalt? So do many of our cold waters above enumerated.

5. Is ſilver, boine in the pockets during a courſe of Aix la Chapelle waters, tarniſhed? The like effect hath been obſerved in ſeveral of our cold waters.

6. Do the waters of Aix la Chapelle yield flowers of ſulphur? and do ſome of the ſprings of Borſel precipitate a magiſtery of ſulphur? The like hath been obſerved in ſome of our cold waters above enumerated.

7. Are the baths of Aix la Chapelle of known efficacy in the cure of the itch, impetigo, vitiligo, and ulcers? So are the cold waters above enumerated, as appears from the ſignal ſucceſſes, which have attended their uſe, even in ſome of the moſt inveterate and rebellious diſorders of this kind.

Is there any raſhneſs then in concluding, that theſe our cold waters do alſo contain ſulphur ſubſtantially diſſolved in them, and differ from the hot ones of Aix la Chapelle and Borſel in nothing but heat, and the different proportions of impregnating minerals of the ſame quality?

Hence appears the great uſefulneſs of examining mineral waters in concert; for as various accidental circumſtances give occaſion to different appearances, the examination of a competent number and variety of them, helps to ſupply the defects in the hiſtories of ſome of them. And, if this were further proſecuted, I doubt not but ſulphur in ſubſtance might be obtained from ſeveral of them, as well as from Aix; which is therefore recommended to phyſicians and naturaliſts.

So

So much may fuffice concerning fuch waters, wherein fulphur is the predominating ingredient; but there are others, wherein there are ftrong indications of its prefence in a fmaller proportion, and mixed with other minerals; and indeed, perhaps, few waters are without an admixture of it; for, befide feveral plain waters, efpecially fuch as contain the native alcali, and the purging waters, fea-water, the brine-fprings, and the chalybeate waters, all which mani-feft a fetor by putrefaction, and fome of them there-upon the like difcolourations of metals as the fulphu-reous waters, the chalybeate waters, in particular, manifeft a fulphureous admixture, by the cream, which they throw up to the furface, the various co-lours whereof, and its difcolouring metals, are marks of fulphur.

There are, moreover, feveral other waters, even fome of thofe, which otherwife make the neareft ap-proach to pure element, having very little falts or earth, which I have mentioned in the beginning of this work, which alfo give ftrong fufpicions of fome degree of a fulphureous impregnation, by the purple and black fediments precipitated from them by fo-lution of filver; which are eminently confirmed by a late examination of the celebrated Holy wells at Malvern, publifhed in the 50th volume of the Tranf-actions, and there extolled for many cures; which, altho' they do not yield quite a grain of folid contents from a pint upon evaporation, give three evidences of fulphur.

1. The purple powder precipitated from them by folution of filver. 2. In exhaling the water flowly in a filver veffel, the bottom of the veffel was tinged

8   of

of a pale yellow colour, as if it had been gilded. 3. When exhaled almoſt to a dryneſs, it emitted vapours of the ſmell of burning brimſtone.

And laſtly, beſides all theſe, I have frequently remarked of ſeveral other waters, here and there mentioned in this work, that their ſediments, obtained by evaporation, did manifeſt ſome pittance of this mineral, by the fetor they acquired on being rubbed with ſalt of Tartar; which, in the language of our author above-mentioned, being attracted by the acid, the phlogiſton is let looſe.

I have above recommended the farther inveſtigation of ſulphur as a deſideratum in the hiſtory of mineral waters; and ſhall now beg leave to conclude this paper with the mention of two or three more articles, which greatly want further elucidation.

The firſt is alum, which, although hitherto found but in extremely few waters, and chiefly in thoſe of Nevil Holt, Ballycaſtle acid water, and perhaps in the Hartfell water in Scotland, above-mentioned, according to ſome late experiments; yet the genuine cryſtals of alum have not as yet been ſatisfactorily demonſtrated in any of theſe.

The ſecond is the volatile mineral alcali, which the nitre of the ancients contained; and ſome of our mineral waters here and there give ſtrong ſuſpicions of, by the experiments with a mixture of quick-lime, and of the ſolution of mercury ſublimate corroſive: but this matter greatly merits farther inquiry, and ſeveral difficulties attending it remain neceſſary to be explained.

Thirdly, the hint lately given in the 49th Vol. Part. II. of the Tranſactions, of the efficacy of the

Carolſbadt water, as ſuperior to lime-water, in diſ-
ſolving ſtones out of the body, if confirmed by cor-
reſpondent events in the internal uſe, would be a diſ-
covery of the greateſt moment, and highly deſerves
to be proſecuted.

I ſhall only obſerve here, that the principal mine-
rals impregnating it are a native alcaline ſalt, and a
calcarious earth ; and that the Aix la Chapelle waters
are not without ſuch a ſalt and earth alſo, and, which
is of more moment, they are reported, by long tak-
ing, to render the urine alcaline, even as do the Ca-
rolſbadt waters; and we are alſo told, that calculi,
macerated twenty-four hours in the water of Aix la
Chapelle, have been reduced to a ſand, or ſoft con-
ſiſtence : but how far this laſt, the Selters, the Bourne
waters, or our Tilbury water, or others alike impreg-
nated, may participate of a like virtue, muſt be de-
termined by further obſervation and experience.

Dublin, the 24th 3d mo. 1759.

XXIX. *An Account of the Effects of a
Storm of Thunder and Lightning at* Rick-
manſworth, *in* Hertfordſhire, *on the* 16th
of July, 1759 : *In a Letter from Mrs.*
Anne Whitfeld. *Communicated by Mr.*
John Van Rixtel, *F. R. S.*

SIR,   Rickmerſworth, Oct. 22. 1759.

MY ſon not being at home, I have
taken upon me to comply with
your requeſt, in giving you an account of the damages
we

we fuftained by the thunder and lightning on the 16th of July laft. It was about a quarter after eight in the morning, when I had but juft got out of the window where I had been fitting, and which is in the weft fide of the houfe, and was not got half-way crofs the parlour, before aviolent ftorm of thunder and lightning burft in at one of the fafhes, and broke five large panes, and tore down the fhutter, and fhivered the window-feat, that the fplinters paffed by me to the farther end of the room ; and one fide of me was covered with the glafs : one pane alfo was broke to pieces where I had been fitting. I had the mortification to ftand fo near the window, as to fee all the tiles and a chimney fhoot off from the houfe, as if fhot out of a fkuttle : the noife was fo great, that at firft I apprehended the drum of my ears broke, and it was fome time before I was able hear at all, and fome hours before I perfectly recovered my hearing. The fulphureous fmell was fo great, I cannot defcribe it, and the heat I felt on my cheek and head not to be conceived, without the fame being felt. When it had ceafed, I was going out at the door, and was met by my daughter, with her comb-tray under her arm, and fplit ; who in a mournful tone faid, Mamma, I am almoft killed. On her telling me it had broke into her room, I immediately went up : I two or three times attempted going into her chamber before I could venture, and then not without covering my face with a thin apron I had on ; the fulphur-fmoke and duft being fo great, that I was almoft fuffocated, befides it being fo darkened by it that I could not fee the window, which I felt out, and fet open. When the duft and fmoke had a little fubfided, how deplo-

rable

rable was the fight to me, and feveral of our neigh-
bours! who by that time were come in. To give you
fome faint idea of it, tho' beyond my defcription, it
had burft into one fide of the room, where there had
formerly been a window, now blocked up, but the
iron-bars left ftanding, which were all forced down,
and a very large hole made through the bricks, and the
timber fplit, a fplinter being carried crofs the room,
and ftuck into the lead of a fmall window, that gave
light to a paffage; the bed driven at leaft two foot
from its place; the rail the vallance were nailed to
fplit in funder; all the vallance were unnailed, and
the rings torn off from the curtains, and fome of the
tefter torn; the locks of a bureau and corner-cup-
board forced open, with the bolts ftanding upright;
fome pictures were broken, and a little India cabinet
broken to pieces, and almoft every thing that was in
the drawers of it: there was a fmall ftand, with a wafh-
hands bafin on it, and a decanter of water clofe to it;
the bafin was broken to pieces, and the decanter not
hurt: the window was fhattered to pieces, and the
hangings of one fide the room torn. My daughter was
in the room at this time, and not hurt, any more
than forced againft the bed-poft, fuffocated with ful-
phur, and almoft deaf as I was: it was thought her
comb-tray preferved her arm, by keeping it hollow
from her waift, as that was fplit, as if knocked in on
one fide with a hammer; and the door, very fortu-
nately being open, gave fome air, which otherwife could
not be had, as there was no chimney in her room,
and, as fhe faid, affifted her in making her efcape
the fooner; for fhe only felt out the door; fo that, had
it been faftened, as it was two or three minutes before

3   this

this happened, fhe would have been fuffocated be-
fore fhe could have got out : a memorable providence
of the Almighty! which fhe and I ought always to
be thankful for.   I then went into the next chamber
on one fide of hers, where the chimney had fallen
from, that lay open to the air : a large ftone round
the chimney, inftead of marble, hung dropping,
which was forced to be taken down by two or three
men, to prevent its breaking the marble hearth:
the chimney-board and a brafs hearth broken to
pieces, as was the wainfcot over the chimney: fix
panes of the fafh-window were broken.   I then re-
turned from viewing the mifchief done up-ftairs, and
went into the garden; the door into it had a large
piece of ftrong, found oak fplit off, and carried away:
a ftone fixed into the gravel, to receive the water
from the fpouts, was thrown upon its edge, and
fmoaked like a boiling pot, occafioned, I fup-
pofe, from being wet with the rain, that had fallen a
little before the thunder-clap, that did this damage:
about ten yards from this ftone, the gravel was torn
up as if with a plough-fhare.   A little garden, that
had two gates oppofite each other; the one, in the
fame weft front as the houfe, was fplit to pieces; and
the leaden fpout, that went down the fide of the gate,
was beat flat, and the ground round it thrown up : the
other gate had its eell torn quite up : the lead over the
door and windows of that fide of the houfe rolled up
like a fheet; and, what is more amazing, the cham-
ber, over the parlour that was fo much damaged,
hurt ; yet the two beyond it received fo much damage
as I have before related ; and none of the rooms under
the aforefaid chambers were in the leaft hurt. Thefe,

Sir,

Sir, are the particulars of that day's misfortunes; but there were some more trifling damages, too much for this present time to admit me to recount: and this account you may depend on as authentic, as more than an hundred people can testify, that flocked in on that, and for several days after, to view. If this is any-wise satisfactory to you, it will give great plea-sure to,

<div align="center">

S I R,

Your humble servant,

Anne Whitfeld.

</div>

XXX. *An Account of some extraordinary Effects of Lightning, in a Letter to Dr.* Gowin Knight : *By Mr.* William Moun-taine, *F. R. S.*

<div align="center">

Dear Sir,

</div>

Read Nov. 22, 1759. **T**HE following account of the effects of lightning, in my neighbourhood, I have drawn up for your perusal; and, if it meets with your approbation, be pleased to communicate the same to the *Royal Society,* or dispose of it in any other manner, as you shall think proper.

During the morning of July the 16th last, was much thunder and lightning : about eight o'clock was heard an extraordinary loud crack, which seemed to me very near, as the large flash and sound were almost coincident. In a few minutes, there was an
<div align="right">alarm,</div>

3.

alarm, that an houfe was on fire in *Goat-ftreet.* I readily fufpected the caufe, and foon after went to the place, in order to inquire into particulars, and was informed, that three houfes were damaged in that place.

The firft houfe I entered into was inhabited by Mr. William Loft, a cuftom-houfe waterman, which was almoft untiled on the weft fide, and being of timber, was very much fplit and fhattered : fome of the weather-boards were thrown outwards to the bottom of the garden, to the diftance of about thirty feet from the houfe, and the windows were forced inwards; but no damage was done to this houfe by fire.

It may not be amifs to mention, that feveral fmall pieces of glafs, in the leaded windows, were impelled with fuch force, as to ftick very faft in a door which was oppofite, and in the hard plaiftered partition ; fome of which I drew out of both, which, together with fome bits of melted window-lead, you will find in the box N° 1.

The fecond houfe, that I examined, was that of Mr. Arthur Tawke, a fail-maker, on the oppofite fide of the ftreet, to the eaftward. This was the houfe faid to have been on fire ; and here I found the following accidents and effects; viz. the window-fhutters of the back parlour, on the eaft fide, were fhattered, and moft of the bell-wire in this room melted; its track on the wainfcot much fcorched, but more fo at the cranks : a hole was burnt in a copper-plate print, which hung under the wire; and, along the fame fide of the room, feveral rufh-bottomed chairs were burnt in fpecks and holes of dif-
ferent

ferent forms and fizes : the floor had the like marks of burning, more efpecially under the courfe of the wire : on the projections of the wainfcot, I found feveral granulations, and longer pieces of the wire, fome of which were bedded in burnt cavities. A few of thefe, taken out by hand, I examined ; but could not find, that they were impregnated with any magnetic quality.

I afterwards employed magnets in fearch of the iron particles, which were found in the crevices of the wainfcot, feams of the floor, and in the bottoms of the chairs, &c. which you will meet with in the box N° 2.

The fervant maid was ftanding in the door-way, between this room and the fore-parlour, when the ftroke happened, and gives. this very fimple defcription of what fhe faw; viz. " that the appearance " in the room was like a fhower of fire."—The difperfion and fall of the red-hot particles of the melted wire would make fuch a reprefentation very natural.

In the fore-chamber, up one pair of ftairs, which lies to the weft, Mrs. Tawke was in bed, having lately laid-in : the flafh alarmed her much; but, having recovered from the fright, fhe perceived a fulphureous fuffocating fmell.—By her direction, a dark clofet near the bed-fide was examined, and found full of fmoke and flames, which were foon extinguifhed. Hence arofe the before-mentioned report of an houfe being on fire.

In this clofet, I found the bell-wire, coming from the parlour below, to be intirely melted, or difperfed, but the effect ceafed at the crank, which tranfmitted it to the chamber adjoining, where it remained intire.

A

A pair of ſtriped cotton trowſers, at the diſtance of 4 or 5 feet from any part of the wire, were burnt almoſt to tinder, a piece of which is here preſerved in the box N⁰ 5.

An old wig-box was burnt in part: a ſheet was burnt through ſeveral folds, in large holes, and alſo a blanket; but the holes were ſmaller, tho' of different ſizes: the velvet cape of a ſurtout coat, at a greater diſtance from the wire, was treated in like manner; tho' the burnt ſpots were in general ſmaller. Hence it ſeems, as if the particles of the fuſed wire did not *all* drop perpendicular; but that they were actuated by ſome impellent force, and that the ſmaller granulations were diffuſed to a greater diſtance; and hence aroſe that appearance of the ſhower of fire before-mentioned: and in this I am ſomewhat the more confirmed by ſome of the facts hereafter deſcribed. Even ſome of the larger pieces were thrown to a great diſtance; for here I found a wire mark burnt in the floor not leſs than ſix feet from any part of the ſuſpended wire, and, on comparing a piece, which the maid picked up in the ſaid place, with the ſaid mark, it appeared to be the ſame, which produced that effect.—You will find it, being the largeſt in the box N⁰ 3.

A deal box, ſtanding on a cloaths-cheſt under the the wire, was burnt in ſpots even more remarkably than the floor, according to the figures and forms of the ſeveral pieces, particles, or granulations. In this cloſet I alſo employed the magnets, and collected from the crevices, corners, &c. a quantity of iron particles, which are contained in the box N⁰ 3.

The third houfe, inhabited by Mr. Robert Harris fenior, corn-factor, lies at the north-eaft corner of the fame ftreet, at the diftance of about 40 yards. Here I found fome damage done to the glaffes and China ware in a clofet contiguous to one of the bell-wires in a ground room: moft of the wire in this room was melted; and a piece of the deal moulding, nine inches long, covering the wire, and adjoining to the brafs thumb-piece, very near the faid clofet, was fplintered off, and ftruck the fervant maid in the face, as fhe was entering the room, at about 14 feet di-ftance.

In fome of the rooms in the fecond ftory, the wires were in part melted. In one room of the third ftory the wire was intirely diffipated; the wall fcorched; the whole plaiftering over the door, ad-joining to the bell, driven out in a body; the floor burnt; and the fheets and quilt of a bed, near the bell-wire, fcorched and fire-pitted in like manner as at Mr. Tawke's; only the effects of the ignited par-ticles were not fo general through this houfe, nor was any thing here abfolutely fet on fire.

I was afterwards informed, that tho' all the wires were not deftroyed, yet they had been obliged to re-new the whole; for, when they came to be exa-mined, they were found fo unpliable and brittle, as to be rendered quite ufelefs.

On the 28th of July I went again to Mr. Tawke's, who, being then at home, conducted me to a garret, which lies partly over the before-mentioned dark clo-fet, from whence a bell-wire was directed to this room, by me unobferved before; nor did the family very foon difcover, that this wire was intirely melted, and the partition greatly fcorched.

This

This room contained a large heap of oats, and another of beans, a foot-path, as it were, being referved next the partition, where the afcending wire ran, which was covered with much duft and ftraggling grains of the corn.—Here again I employed the magnets, Mr. Tawke affifting, and collected a confiderable quantity of melted particles, not only from the floor, joints and projections of the fkirting-board, but from the holes and chafms in the broken plaiftering, where-ever we could introduce the magnets, and that at the diftance of 4 or 5 feet from the wire-place. Thefe you have in the box N° 4.

Hence I fufpected, that *granulæ* might be found among the oats : we probed, but to no no purpofe; the heap was too large, and had been moved, by fetching away what was wanted from time to time. However, we carried our inquiry to the bare floor, quite acrofs the heap, at 10 feet diftance, and, in the joints or feams, found of very fine particles a fufficient quantity to prove, that they were violently diffufed to a great diftance.

From thefe houfes the lightning feems to have tended towards the N. E. for in that direction, at the diftance of about 200 yards, and not fo far from my own houfe, Captain William Provoft was ftruck thereby, ftanding in his own entry, and rendered almoft fenfelefs and fpeechlefs for fome hours, and, for feveral days, was much afflicted with a ftupor, giddinefs, and vomiting, and retained a conftant and ftrong tafte of fulphur in his mouth and throat. His child had hold of his cloaths, and his wife was near him ; but the ftroke appears to have been above the child, as it feems to have ftruck him about the head. No other damage was done here.

Mrs.

Mrs. Provoft fays, that the flafh or fire feemed to be as large as a fmall pewter plate, and paffed clear through the entry (the doors being open) directed to the northward.

On the oppofite fide of the ftreet, and fomewhat oblique towards the north-eaft, at the diftance of about 20 yards, Mr. Ambrofe Lyon, fail-maker, had, at the fame time, about fix dozen of bottles of Port wine broken to pieces.

The front of Mr. Lyon's houfe has nearly a fouth-weft afpect: among other conveniences under-ground is a fubftantial arched brick vault, and capacious for a private houfe; it is quite clofe, having no light or opening into it, but at the door, which faces the fouth, and is always kept locked.—On the weftern fide of this vault were repofited feveral cafks of wine, moft of them iron-bound; alfo feveral dozens of rum behind them upon the floor. On the northern end was a circular tub or cooler, iron-bound with three hoops, containing Port wine in bottles. About ten feet from hence, on the eaftern fide, nearer the door, and directly oppofite thereto, ftood by itfelf another large circular cooler, 32 inches diameter, and 12 inches deep, bound alfo with three iron hoops, containing about fix dozen of Portugal wine, called *Barabarba*, the remainder of a larger quantity, which he had kept in cafk for more than two years before it was bottled, in which laft ftate it had been for more than three months, and all proper care taken of it; for he is very curious in thefe things. Thefe bottles were inclined upon their fides, for their better prefervation, as were all the others containing wine; and this tub was fixed upon fkids (pieces of timber) about fix inches thick.

In half an hour, or lefs, after the great thunder-clap, having occafion to go into the vault, upon his entrance, he immediately difcovered the noife of a running drip, and liquor flowing about the floor, which, upon further infpection, he found proceeded from the laft-mentioned cooler, the bottles therein contained being broken to pieces, as if done by a mallet, not fo much as one remaining intire of the whole quantity; and the cooler being not very tight, the wine was running out from thence; but, by a quick application of fuch veffels as were at hand, the remainder was preferved, and, after clearing it from the glafs, was put into a fmall cafk to recover, adding thereto about a one-third part of Port wine to fill up the veffel, fome of which I have tafted; and a bottle, drawn from the faid cafk, I herewith fend you.

Mr. Lyon did not obferve, at the fame time, any kind of alteration in the faid wine made by ferment-ation, or otherwife; only that it was vapid or flat: one bottle of the faid wine was afterwards found among the bottles of Port in the firft-mentioned cooler, which was not affected, but was fine, and in perfection; nor was any other damage done in the vault, that he could difcover upon the ftricteft inquiry.

Captain John Dickinfon, a gentleman in the neighbourhood, fhared half the cafk of the faid wine, which had a fimilar management, was bottled off at the fame time, and had a lefs cool and friendly vault; yet not one bottle of this has hitherto been in anywife damaged.

I have

I have been more particular in the examination of fome of the foregoing facts, as they feem to contradict an opinion generally received, that the folution of metals by lightning is effected by a kind of cold fufion; for it appears very evident, that the melted iron-wire, in the feveral preceding cafes, had all the marks of heat and ignition, that ufually attend the fufion of that metal, when brought about by common fire. I am,

Dear Sir,

Your moft obedient fervant,

Gainsford-ftreet, Southwark,
Septem. 28th, 1759.

Wm. Mountaine.

Some Remarks on the preceding Letter, by Gowin Knight, M.B.F.R.S. and Principal Librarian of the Britifh Mufeum.

Read Nov. 22, 1759. THE facts, contained in Mr. Mountaine's letter, are an evident proof, that the fufion of metals by lightning is, fometimes at leaft, attended with heat and ignition, as in the cafe of common fufion. And, fince the reading of thofe facts, I have been more and more induced to fufpect, that the received opinion of a cold fufion is a vulgar error, tho' too generally adopted, and of very long ftanding. From fome of the circumftances attending thefe facts, compared with what is to be found in authors relating to the fame fubject, I think it poffible both to fhew, whence this opinion firft took its rife, and how it became fo general; and at the

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the fame time to prove, that there is no clear evidence for the truth of it from any relations hitherto publifhed.

The inftances, that are moft generally given of cold fufion, are two; that of a fword being melted in its fcabbard, and that of money being melted in a bag, both the fcabbard and bag remaining unhurt. A great number of authors have mentioned both the facts, without giving their own teftimony, or that of any one elfe, for the truth of them, or defcribing any of the other concomitant circumftances. However, it feems poffible, that lightning might produce effects fufficiently fimilar to thefe, to give rife to fuch reports, without our being obliged to have recourfe to a cold fufion to account for them.

If at any time the edge or external furface of a fword had been melted, whilft the main part of the blade remained intire, it would have afforded fufficient grounds to affert, in general terms, that the fword was melted, and yet the fcabbard might have remained unhurt; becaufe either the edge or furface of a fword might be inftantly melted by lightning, and cooled fo fuddenly, as to make no impreffion of burning on the fcabbard. Metals, as well as other bodies, will both heat and cool fooner, in proportion as they are thin and flender. Very fmall wire will inftantly become red-hot, and even melt, and run into a round globule, in the flame of a common candle; and it is no fooner removed out of the flame, but it is as inftantly cool. The edge of a fword therefore, or even its furface, might be inftantly melted by lightning, and being in contact, or rather ftill united to the reft of the blade, which might be ftill
cool,,

cool, it would part with its heat too suddenly to produce any appearance of burning.

I was confirmed in this reasoning, by examining the fragments and melted particles of wire sent me by Mr. Mountaine. Amongst them there appeared to be globules of various sizes, which had undergone very different degrees of fusion : the largest of these had not been fluid enough to put on a spherical figure ; but they approached nearer thereto, in proportion as they were smaller : so that in the smallest *granulæ* the fusion was most perfect, the globules being very round and smooth. Their sizes continued diminishing, till they became invisible to the naked eye ; and some of them, when viewed with a microscope, required a third or fourth magnifier to see them distinctly.

Some of the bits of wire were rough and scaly, like burnt iron, and were swelled in those places where they were beginning to melt : others continued strait, and of an equable thickness ; but their outward surface seemed to have undergone a perfect fusion, so that there were two or more pieces adhering together, as if joined by a thin solder.

In Mr. Pitfold's account of the effects of lightning at Darking in Surry, published in the Philosophical Transactions *, mention is made of a similar fact. He says, " some small tacks were soldered together, six, " seven, eight, or ten in a clump, as if they had " had scalding metal run over them."

It is easy to conceive, how the heat of this superficial fusion might be so suddenly diffused throughout

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* Philos. Tranf. Vol. XLIX. p. 311.

the

the metal it furrounded, which the lightning might not have heated, as inftantly to have reduced the whole to too cool a ftate, for any other contiguous body to be burnt thereby.

In like manner, a ftream of lightning paffing thro' a bag of money, might fufe the furfaces of fuch pieces as lay in its way, and folder a number of them together ; and yet the bag remain unhurt.

An accident or two of this kind may have come, by tradition, to the knowlege of fome of the firft collectors of marvellous facts, and from them be tranfcribed by others, perhaps with additions and improvements. Thus, according to Pliny*, both gold, filver, and brafs, have been melted in bags fealed up, which were not in the leaft burnt, nor was the wax of the feals melted : whereas Seneca † fpeaks only of filver being melted in the pocket or purfe, which remained whole and unhurt. Later writers feem to have copied from one of thefe for the moft part, without mentioning their authority.

In the Philofophical Tranfactions are two or three relations, which feem, at firft, to favour a cold fufion; but, when duly confidered, prove nothing conclufively. The firft is in a paper concerning the effects of lightning at Colchefter, on July 16, 1708; which concludes with obferving, that, " during the fame " ftorm, four perfons were killed in a boat, that was " going from Harwich to Ipfwich ; and that, in one " of their pockets, a watch and chain was melted all " on a lump." In another account, given by Or-

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* Plin. Nat. Hift. lib. ii. c. 51.
† Seneca, Nat. Quæft. l. ii. c. 32.

lando Bridgman, Efq; defcribing the fame ftorm, it is faid, that " the chain of the watch was melted, " and that no harm or burn could be perceived on " his breeches or cloaths." Now, if both the watch and chain were melted all on a lump, and the pocket unburnt, as might be concluded from both thefe accounts laid together, it would be a ftrong argument in favour of cold fufion : but there is great reafon to fufpect the truth of the firft-mentioned relation ; becaufe the author of it writes only from hearfay, being himfelf at Colchefter, at feveral miles diftance from where the thing happened. Whereas Mr. Bridgman was upon the fpot, and examined one of the dead bodies himfelf very minutely; and tho' he does not fay, that he faw the body of him, whofe watch-chain was melted, yet he gives a very circumftantial account of it ; and, if the watch had been melted, as well as the chain, he could not have omitted that particular. It is therefore probable, that the chain only was melted, and that, hanging out of the pocket, it had left no marks of burning on the breeches.

We have, in our Tranfactions, another account of the effects of lightning by Dr. Cookfon, of Wake-field †, who relates, " That the lightning fell on a " box of knives and forks, and melted a great many " of them, the fheaths being untouched." But the doctor, in another account, which is fuller and more exact, fays, " the lightning difperfed a great " many dozen of knives and forks, which were put up " in a box, all over the room. Upon gathering them

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* Philof. Tranf. Abr. Vol. V. p. 154.
† Ibid. Vol. VIII. p. 504.

" up, fome of them were melted; others fnapped in
" funder; others had their hafts burnt; others their
" fheaths either finged or burnt; others not." From
all which circumftances, duly confidered, I think no-
thing certain in favour of cold fufion can be fairly
drawn.

XXXI. *An Account of a Meteor feen at*
Shefford, *in* Berkfhire, *on* Saturday, Octo-
ber 20*th*, 1759; *with fome Obfervations
on the Weather of the preceding Winter:
In a Letter to* Thomas Birch, *D. D. Sec.
R. S. from* Richard Forfter, *M. A. Rec-
tor of* Shefford.

<div align="right">Reverend Sir,   Shefford, Octob. 31, 1759.</div>

Read Nov. 8,
1759.,
ON Saturday the 20th inftant, about
Six in the evening, a ball of fire
fell nearly eaft from this place. I did not fee it my-
felf. My fervant (who is a very fober, honeft fel-
low) fays it was nearly of the fame fize with the
moon, and full as bright as fhe ever fhines: its mo-
tion was very fwift, and, as far as he could judge
(for it was out in a moment) quite downright, i. e.
perpendicular to the horizon.

And now my hand is in, I cannot forbear acquaint-
ing you with an obfervation I have made, which bids
fair to overfet a maxim pretty ftrongly eftablifhed in
the world, as not being only believed and depended
on by the vulgar and middling people, but mentioned

as such, I think, by several authors. In short, the maxim is this; viz. that a plentiful year of mast is an infallible prognostic of an hard or severe winter. Now, it happened last year, that provisions of this sort were as plentiful as ever was known; the trees and hedges being loaded in such a manner, as to bend and break under the pressure of their own weight: and yet the winter was the mildest, perhaps, that ever happened in this country: and accordingly not one quarter of nature's store was consumed. We had no ice, but once, and that not the thickness of an half crown, which did not continue 24 hours. I see by Cuff's tables, published in a monthly paper, that, in London, the thermometer was never below 32; and so low as this but twice, and then only by starts. I had ranunculus's in full bloom from the middle of December to the middle of February, and they not sheltered, but by a wall north, 25° east. In the middle of January, I had self-sowed marigolds and violets in bloom. Jan. 15, the bees roared, and were as busy as they are in the height of the working sea-son: and Jan. 18, the birds sung as chearfully as they generally do in May.

It seems probable to me, that the great abundance of berries and wild fruits (by which I mean mast) is intirely owing to a very backward spring; for, when the blossoms do not open till pretty late in May, they are secure from those inclement blasts, which, when they unfold themselves sooner, do pinch and blight the greatest part of them. I am,

<div align="center">

Reverend Sir,

Your affectionate brother,

and most obliged humble servant,

Richard Forster.

XXXII. *An*

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XXXII. *An Account of the same Meteor,
seen at* Bath: *In a Letter to* Tho. Birch,
D. D. Sec. R. S. from Mr. Josiah Cole-
brooke, *F. R. S.*

Dear Sir,

Read Nov. 15, IN compliance with the president's de-
1759. sire, the following is the account of
the meteor seen at Bath.

On Saturday the 20th of October, between five
and six in the afternoon, as I walked over the north
parade, a ball of fire, of the bigness of a tennis ball, of
a very bright colour, with a train of four or five feet
in length, darted from the north-west, and, describ-
ing the arch of a great circle on my left hand, sunk
behind the hills to the south-east: just before it sunk,
several large sparks of bright blue fire issued from it;
but it did not seem to burst: it was not more than
two seconds in its passage, and I could compare it to
nothing, but the most glorious sky rocket I had ever
seen.

Mr. Peers, a gentleman of London, was at Bath
at the same time, and being in a room fronting the
east, that looked over the meadow between Bath and
Bathwick, he told me, with some surprize, the next
day, that he saw the largest star he had ever seen,
fall into the meadow; and, what was most particu-
lar, that it fell perpendicularly; whereas all he had
ever seen before shot obliquely in the sky.

This must have been a spark from this meteor, as
the time he saw the star agreed with the time I saw
the ball of fire.

I do

I do not remember, that I heard any noife or whizzing in the air as it paffed; nor did any fulphureous fmell attend it, that I could perceive.

I looked on my watch immediately after it was gone by, and found by that, it was juft twenty minutes paft five; but, as I could not be fure that went right, I chofe to mention the time in more general terms.

It was feen by vaft numbers of people, and much talked of the next day.

I am, dear Sir, with much efteem,

Your very humble fervant,

Budge Row, Nov. 14, 1759.   Jofiah Colebrooke.

XXXIII. *An Account of the Meteor feen at* Chigwell Row, *in* Effex, *on the* 20*th of* October 1759: *In a Letter to the Rev. Dr.* Birch, *Secretary of the Royal Society, from Mr.* William Dutton, *Watchmaker in* Fleet-ftreet.

S I R,   Fleet-ftreet, Dec. 1, 1759.

Read Dec 6, 1759. IN compliance with Dr. Pringle's requeft, I fend you the following account of the meteor, which I faw fome time ago, viz. on Saturday, Octob. 20th, about a quarter before fix in the evening, whilft there was ftill fome day-light, tho' feveral ftars had begun to appear.

At that time I happened to be walking with my face eaftward, with a companion, at Chigwell Row,

8   in

in Effex (which lies about 12 miles eaft of London, and upon a pretty high hill), when we obferved a meteor bearing northward of the eaft, in appearance not high up in the air, tho' with a confiderable angle of elevation, perhaps of about 70 or 80 degrees, with its declination from the zenith eaftwards. It moved with great velocity, in a direction from north to fouth, and feemingly in a curve line downwards; but vanifhed at the height of 4 or 5 degrees above the horizon, then bearing nearly fouth of us. It was of a round form, about the fize of the planet Venus, when feen at the largeft, of a light bluifh caft, but very bright. At its vanifhing, feveral particles, ftill brighter than itfelf, and fomewhat like the ftars, that are feen upon the breaking of a rocket, feemed to iffue out of it. We perceived a faint light to follow it, like the tail of a comet, and about two feet in length. The whole time of the appearance did not exceed 3 or 4 feconds. This is the beft account I can give of what I faw; and my companion, to whom I have fhewn this letter, agrees with me in all thefe circumftances. I am,

SIR,

Your very humble fervant,

Wm. Dutton.

XXXIV. *An*

XXXIV. *An Account of Two Stones of re-*
markable Shapes and Sizes, which, for the
Space of Six Years, were firmly lodged in
the Urethra *of a young Man, and at length*
successfully cut out from thence. Addressed
to the Royal Society, on Thursday Decem-
ber **13, 1759,** *at which Meeting the Stones*
themselves, and a Drawing of the Stones,
were presented to the Fellows of the Society,
by Joseph Warner, *F. R. S. and Surgeon*
to Guy's Hospital.

Read Dec. 13, 1759. THE formation and confinement of stony concretions in the different parts of the organs of urine, to wit, in the kidneys, ureters, bladder, and urethra, are diseases, which are frequently observed to happen to both sexes of all ages and constitutions: for this reason I am apprised, that there are few instances can be given of such peculiarities attending these cases, as may reasonably be esteemed worthy the attention of the curious. But when matters of fact, however common in themselves, are so circumstanced, as to assume extraordinary appearances, the uncommon phænomena accompanying such facts, when capable of being pointed out, will, I believe, be always considered by the Royal Society, as a sufficient apology for the freedom of the communication. Upon this presumption, I have taken the liberty of laying before the Fellows of

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this Society the following fhort account, and, at the fame time, of fubmitting to their infpection as much of the cafe, as the nature of the affair could admit of being exhibited.

C A S E.

Thomas Bingham, a very healthy young man, 20 years old, came from Yarmouth to London, in September 1759, and put himfelf under my care, to be cured of a fwelling; which he had in his urethra. Upon queftioning the patient, I was informed by him, that he had little or no pain; that he had never perceived the leaft difficulty in voiding his urine, nor had he ever had the leaft involuntary efflux of it : he had not at any time fuffered in the leaft but from the bulk and weight of fomething that grew in his urinary paffage (urethra), which, upon exercife of late, proved troublefome to him. Upon infpection, I difcovered a confiderable prominence betwixt the tefticles and anus. Upon feeling the part with my fingers, there appeared a very evident hardnefs and tumor.

By introducing a large, fmooth, and ductile, probe into the urethra, it was evident there was a ftone or ftones lodged in that paffage. I advifed the cutting the tumor out, which was complied with ; and in the following manner I proceeded to the performance of the operation.

The patient being fupinely placed upon a fteady table, of a convenient height, covered with a double blanket, and a pillow put under his fhoulders, I caufed his hands and feet to be tied together; and, by proper affiftants, he was held in the fame pofition as

is done in the operation of cutting for the ftone in the
urinary bladder. I then proceeded to divide the ure-
thra longitudinally by incifion. The extent of the
incifion was from one end of the fwelling to the
other : the length and fize of the wound enabled me
to take away the ftones without any violence or diffi-
culty.

After the ftones were removed, I brought the lips
of the wound together, and, with the twifted future,
I retained them in that fituation. By this method,
and by occafionally paffing a bougie of a proper fize
into the urethra, beyond the farther extent of the in-
cifion, the patient went happily on, till the cure of the
wound was completed, which was effected in about
three weeks; and there afterwards remained no incon-
venience at all to the patient in voiding or retaining
his urine.

N. B. As the identical calculi are prefented to the
Fellows of the Royal Society for their infpection,
fo that the exact fizes, fhapes, and external forma-
tions, of thefe ftones may be feen; I think it quite
unneceffary to give a written account of thefe par-
ticulars. However, as it may probably give fome
fatisfaction to the curious to be informed of the fpe-
cific gravities of thefe ftones, I have fubjoined
thefe particulars to this memoir, and have like-
wife, for the fame reafon, caufed an engraving of
thefe ftones to be made, and added to this paper.
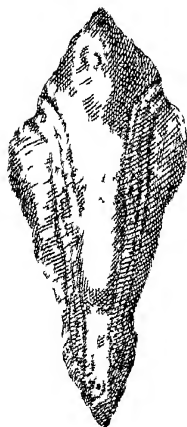
Weight

Fig. 1. *Fig. 2.*

Weight in air.		in water.		Spec. gravity.
Large ftone 308	5	92	85	1.431
Small ftone 42	35	10	4	1.326
Both ftones 350	8	102	9	1.415

Quære. Is it not probable, that the generation of thefe ftones might originally have begun in the urethra, as the patient did not ever remember to have had the leaft complaint in his loins, or in any part of his bladder? Or is it more reafonable to fuppofe, that the ftones were firft of all formed in the kidney, or urinary bladder, from thence conveyed with the ftream of urine, when very fmall, and lodged in the urethra, till they had arrived to thefe fizes?

Either of thefe fuppofitions to me appears reafonable.

However, as hypothefes of this kind are with difficulty afcertained, and as a difcovery of the fact, could it be determinately made, would prove of very little confequence to the improvement of the art of furgery, I think it quite unneceffary to dwell upon a part of this fubject, in which the benefit of mankind appears to be fo little concerned.

Plate VIII. *Fig.* 1. reprefents the upper parts of the two ftones, as they were found lying loofely together in the urethra. The longeft end, that is, the fmalleft of thefe two ftones, was fituated neareft to the neck of the bladder, or orign of the urethra. It may be obferved, that, on the fuperior part of thefe ftones, there are two long grooves or chanels, which were gradually formed by the ftreams of urine, that were occafionally difcharged from the bladder.

It

It may farther be obferved, that the whole fur-
faces of thefe two ftones, except the parts of them
where they lay in contact with each other, or where
they continually preffed upon the lower furface of the
urethra, or bed, which they had formed for themfelves
in this chanel, are rough, and have feveral inequa-
lities, or eminences arifing from them.

Fig. 2. reprefents the inferior fmooth parts of the
two ftones, as they appeared when feparated from
each other, as well as the fmooth or polifhed furfaces
of the ends of thefe ftones, which lay in contact with
each other, and upon motion rubbed againft each
other,

XXXV. *Experiments on the* Tourmalin : *by*
Mr. Benjamin Wilfon, *F. R. S. In a letter*
to Dr. William Heberden, *F. R. S.*

SIR,

Read Dec. 6.
1759.
I Have the pleafure to communicate to
you fome experiments made upon the
Tourmalin, or *Afhftone*, which you were fo kind as to
procure me, together with fome others, refpecting the
vitreous and refinous electricities, as they are called,
and the obfervations I have made thereupon.

The more I am acquainted with electricity, the
more I admire a wonderful fimplicity which feems
to prevail in nature, at leaft in this part which abounds
with phænomena of a very curious kind ; whereof
many that have paffed under my examination of late
are fo extremely nice, that I avoid venturing to relate
them, becaufe I would not willingly fubject myfelf
to the cenfure of incautious obfervers.

I can-

I cannot enter into this particular fubject, without firft fettling a difpute amongft electricians, which fub-fifts at this day, concerning the two electricities; be-caufe fome confequences, drawn from the feveral experiments I have to produce, greatly depend upon it.

Polifhed glafs, upon being rubbed properly, has been fuppofed to *give* an electricity to bodies, and thofe bodies that receive it from the glafs, are faid to be electrified *plus*. Whereas wax, amber, &c. upon being rubbed in the fame manner, have on the contrary been fuppofed to *receive* an electricity from bodies, and thofe bodies which part with it are faid to be electrified *minus*. But no experiment, that I know of, has yet appeared to determine which of thefe electricities does really electrify *plus*, and which of them does really electrify *minus*; though it happens that the fact turns out juft as they have all along fuppofed.

In a fecond treatife upon the fubject of electricity, that I publifhed in the year 1748, feveral experiments were produced, to fhew that all bodies are furrounded with a *medium*, which is *of an exceeding elaftic nature, and extends* but to *a very fmall diftance from the body* when it is not difturbed by heat, or other caufes. Since that, other experiments of the like kind have been publifhed in a work, wherein my late worthy friend *Dr. Hoadly* was concerned with me. Among the proofs therein given, is a curious one, which I obferved in the *Torricellian vacuum*, where the appearance was remarkably fenfible. And what is more fingular, that fame appearance not only proves the exiftence of a medium, at or on the furface of bodies, but at the fame time determines which

of

of the electricities is truly *plus*, and which of them is *minus*.

You muſt remember, *Lord Charles Cavendiſh* firſt obſerved the luminous appearance in one continued ſtream throughout the whole *vacuum* of an exhauſted tube. It is a fine experiment, and affords more information than I at firſt imagined, therefore I ſhall beg leave to recite it from the Philoſophical Tranſactions *, before I give you any farther particulars about it.

" This apparatus conſiſted of a cylindrical glaſs
" tube of about three tenths of an inch in diameter,
" and of ſeven feet and half in length, bent ſome-
" what like a parabola, in ſuch a manner, that thirty
" inches of each of its extremities were nearly
" ſtraight, and parallel to each other, from which
" an arch ſprung, which was likewiſe of thirty in-
" ches. This tube was carefully filled with mer-
" cury ; and each of its extremities being put into its
" baſon of mercury, ſo much of the mercury ran
" out, until, as in common barometrical tubes, it
" was in equilibrio with the atmoſphere. Each of
" the baſons containing the mercury was of wood,
" and was ſupported by a cylindrical glaſs of about
" four inches in diameter, and ſix inches in length ;
" and theſe glaſſes were faſtened to the bottom of a
" ſquare wooden frame, ſo contrived, as that to its top
" was ſuſpended with ſilk lines the tube filled with
" mercury before mentioned ; ſo that the whole of
" this apparatus without inconvenience might be

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* Vol. xlvii.

" moved

J. Mynde f.
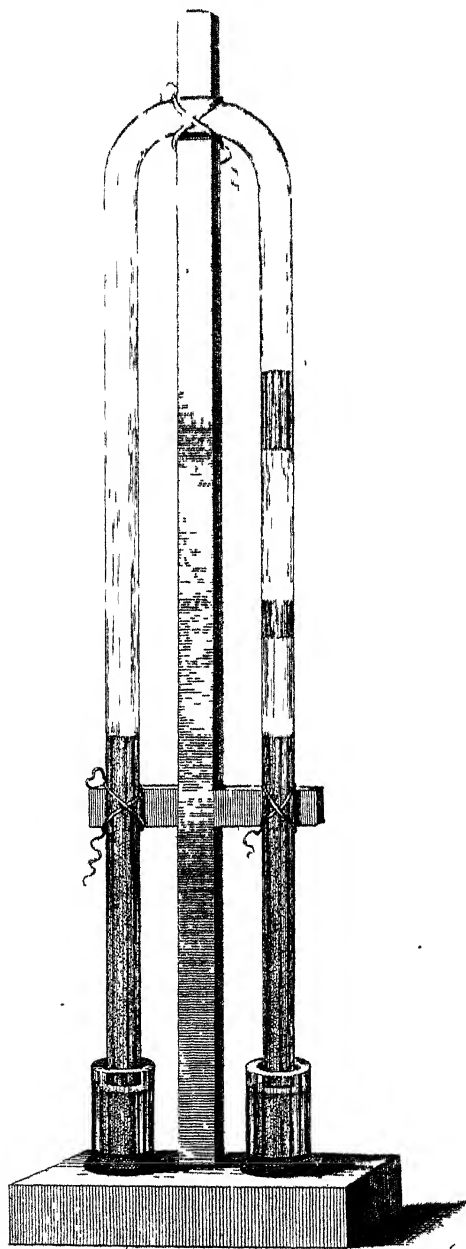
" moved together. The *Torricellian vacuum* then
" occupied a space of about thirty inches. In make-
" ing the experiment, when the room was darken-
" ed, a wire from the prime conductor of the com-
" mon electrical machine communicated with one
" of the basons of mercury, and any non-electric
" touching the other bason, while the machine was
" in motion, *the electricity pervaded the vacuum in*
" *a continued arch of lambent flame, and as far as*
" *the eye could follow it, without the least divergen-*
" *cy*."

I imagine *Dr. Watson*, who has described this ex-
periment, did not, when he made the experiment, at-
tend to a singular appearance of light upon one of the
surfaces of the quickfilver, because he has taken no
notice of it. However, I had a mind to make the
experiment myself, and try whether I could not ma-
nage the quickfilver, so as to have more than two vi-
fible surfaces, in order that I might have that re-
markable appearance repeated.

To do this, I let a very small quantity of air into
the tube, by which means four columns of quickfil-
ver were obtained, reckoning great and small toge-
ther; and six visible surfaces, three of which I called
upper, and three under surfaces. But the figure in
the plate N°. IX. will give you a better idea of the
inftrument, than I am able to do by writing.

When the column of quickfilver on the left hand
was electrified, and the other on the right commu-
nicated with the earth, the stream of light was vi-
fible in a darkened chamber, and the general appear-
ance, all the way through the *vacuum*, was a light of
a feeming uniform denfity, excepting at the upper
surfaces, for above one tenth of an inch, reckoning

<div align="right">from</div>

from each furface; and there the light was always confiderably brighter; infomuch that a gentleman, then prefent, who was unacquainted with experiments of this kind, defired to know the reafon of the three *knobs* of light appearing on the tops of the quickfilver. I mention this circumftance, becaufe they were remarkably confpicuous; whereas the three under furfaces exhibited no fuch appearance, the light being rather lefs bright than even in the general appearance of the whole illuminated *vacuum*.

An electric current, fetting in from the glafs of the electrifying machine, and paffing along the tube through the quickfilver and *vacuum*, and fo to the earth, muft have caufed thofe bright *knobs* by means of the refiftance the fluid met with at the upper furface of the quickfilver in endeavouring to enter it; becaufe the appearances were alike on the three upper furfaces, and nothing of the fame kind was feen at the under furfaces. Glafs therefore electrifies bodies *plus*, or, in other words, *gives* bodies a quantity of electric fluid more than they have naturally.

I am now to acquaint you with the appearances that the *minus* electricity occafioned.

Inftead of the glafs cylinder to electrify with, I put a refinous one, preferving the communication, and every thing elfe, as in the former experiment. In thefe circumftances, the general appearance of the light in the *vacuum* was the fame; but in this experiment the *knobs* of light were on the under furfaces, and not upon the upper.

From the knobs of light being at the under furfaces, and not upon the upper, I concluded that the flow of the fluid was the contrary way to that caufed

by

by glaſs ; for in this caſe it appeared to come from
the earth, then into the tube, and ſo on to the reſinous
cylinder. Thoſe bodies therefore which have no com-
munication with the earth, and from which the reſi-
nous cylinder is ſupplied, muſt be electrified *minus* *.

Theſe luminous *knobs* (were there no other proofs)
are a ſtrong confirmation of the exiſtence of a *medium*
at or on the ſurfaces of bodies, which, to a limited
degree, reſiſts or hinders the entrance and exit of the
electric fluid. But we have ſome other experiments
to illuſtrate the ſame, and the *Tourmalin* does aſſiſt us
not a little.

There is one thing more, which I am obliged to
take notice of, concerning the impermeability of glaſs
by electricity. Our friend, *Dr. Franklin*, ſeems to
found a great part of his ſyſtem on this opinion.

" Glaſs, ſays he, has within its ſubſtance always
" the ſame quantity of electrical fire, and that a very
" great quantity in proportion to the maſs. This
" quantity, proportioned to the glaſs, it ſtrongly and
" obſtinately retains, *and will have neither more*
" *nor leſs*, though it will ſuffer a change to be made
" in its parts and ſituation; *i. e.* we may take away
" part of it from one of its ſides, provided we throw
" an equal quantity into the other."

This doctrine I could never agree to, ſo far as it re-
lates to the impermeability of glaſs, as appears by my
letters in anſwer to the Abbé *Nollet* and M. *le Roy*,
on their requeſting to know my ſentiments about it,

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* The ſame cylinder made rough with emmery, and rubbed with
flannel at one time, and with leather at another, will afford theſe
different appearances, the firſt cauſing the *minus,* and the laſt the
plus appearance.

in the year 1756, the former of whom it is well known could never come into it*. And I am ftill the lefs inclined to alter my opinion of this matter, from an acquaintance with the properties of the *Tourmalin*, which I think has furnifhed me with fufficient grounds for advancing that *Dr. Franklin* is miftaken. I have wondered that *Æpinus* did not take notice of fome of the experiments which electrify glafs either *plus* or *minus*, becaufe the *Tourmalin* affordeth leading experiments towards it; and can afcribe it to no other caufe than a favourable opinion he was willing to entertain of *Dr. Franklin's* hypothefis.

To prove that glafs is permeable by electricity, I took a very large pane thereof (I chofe it large, that no objection might be made to the experiment) and then warmed it a little, fo that it was free from moifture. After that, I held it upright by one edge, whilft the oppofite edge refted upon wax, and rubbed the middle part of the furface with my finger. After this, I found that both fides were electrified *plus*. Upon repeating the experiment many times, with different glafs and different degrees of rubbing, ftill both fides were in every experiment electrified *plus*. Now they could not both be electrified *plus* by this treatment, unlefs fome part of the fluid had really paffed through the glafs; becaufe the virtue could not get round to the other fide over fo great a furface: befides, the extremities of the glafs were not electrified at all. As to electrifying glafs *minus*, I find it may be done three different ways. But thefe and other experiments relating to the permeability of glafs, I fhall

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* Vid. lettres fur l'ectricité par M. l'Abbé Nollet, lett. iv.

forbear

forbear mentioning, till I have brought you better acquainted with the *Tourmalin*.

The firſt account I met with of the *Tourmalin*, and the remarkable properties belonging to it, was from a *memoir in the Berlin aƈts*, printed in the year 1758; wherein it appears, that *F. V. T. Æpinus*, profeſſor of natural philoſophy, made ſeveral very curious and judicious experiments upon it ; the moſt material of which, prove a *plus* electricity on one ſide thereof, and, at the ſame time, a *minus* electricity on the other ſide ; provided the *Tourmalin* is moderately warmed, and even by hot water. Theſe appearances are the more extraordinary, as the like means employed in the ſame manner upon diamonds, glaſs, and all other electric bodies hitherto tried, produce no ſuch appearances.

The *Duke de Noya*, who viſited this kingdom in 1758, wrote a ſmall treatiſe on the ſubject, and publiſhed it at Paris on his way to Italy : in this work he mentions *Æpinus*'s experiments, but does not admit of a *plus* and *minus* electricity belonging to the *Tourmalin* when heated. On the contrary, he ſays that the two ſides are electrified *plus*, but one of them is more ſo than the other; and that it is the difference between thoſe degrees which has led *Æpinus* into the miſtake.

I remember to have repeated moſt of the experiments mentioned in the *Berlin memoir*, ſoon after it appeared in England, with the *Tourmalin* belonging to our friend *Dr. Sharp*, which you fortunately recollected to have ſeen in his poſſeſſion many years ago at Cambridge; for it was the only one known of here at that time : and though but a ſmall one, compared

with

with *Æpinus's*, yet it was large enough to satisfy me that his opinion was well founded. Besides, the trials you made with the same stone before I had it, were a farther confirmation of the truth. However, the following experiments, which I made to procure more *data* towards attaining some simple explanation of these curious phænomena, will sufficiently prove, that one side of the stone is really electrified *plus*, and the other side *minus*. And had the *Duke de Noya* made farther experiments, and pursued the same method I have done, I think he would have been of the same opinion.

The largest *Tourmalin* I had from you, and with which I made the following experiments, weighs above 120 grains. It is of an oval form, and polished: the greatest diameter measures an inch and a quarter, and the least one inch. One side is *plain*, the other is *convex*, but cut into several small planes or *facettes*, something like a rose diamond; the thickest part of which is near one third of an inch. This shape does not seem to me to be the most convenient for making experiments, but I would not alter it, lest the stone should break; for there are several cracks in it; and I fear it will be difficult to meet with another of the same size.

To make experiments with the *Tourmalin* requires the greatest attention, as the appearances sometimes are scarcely sensible, insomuch that I have been obliged to employ the tenderest kind of apparatus, and even interpose a sort of skreen to prevent my breath, or other like motions of the air, from disturbing the experiment.

My apparatus for making many of these experiments, consists of two very small balls made of the

pith

pith of elder, and fufpended by two linnen threads of the fineft kind: the ends of thefe threads I faften to a flip of wood about three inches long, and half an inch broad: then upon a ftick of fealing wax nine inches long, fixed upright on a table, or any other convenient place, I faften the flip of wood, from one end of which the threads &c. hang down five inches, fo that the pith balls are about four inches from the table. Thefe balls are always fuppofed to be electrified *plus*, except where the contrary is mentioned: but with no greater power, than to make them recede from each other about one inch in every experiment.

I prefer the wax ftand to a glafs one, as the latter when cold acquires moifture very foon, and therefore becomes a conductor: whereas wax, when it is once in good order, will continue a non-conductor for a long time.

Before I fet down my experiments, I muft mention three truths that are commonly known, and which, for the prefent, I fhall call

General Laws.

Two bodies equally electrified *plus* recede from each other, or are repelled.

Two bodies electrified *minus* recede alfo from each other, or are repelled.

One body electrified *plus* and another body electrified *minus*, to an equal degree, accede, or move towards each other, or, as it is generally expreffed, are attracted.

Experiments *upon the* Tourmalin.

Exp. 1. One edge of the *Tourmalin* being properly faftened to a long ftick of fealing wax, I dipped the
ftone

ftone into boiling water, and continued it there near one minute. On taking it out, and prefenting the *convex* fide thereof near the pith balls, they immediately *receded* from it, but not very ftrongly.

On turning the *plain* fide towards the balls, it caufed them to *accede* to it, but rather at a greater diftance in this cafe, than they *receded* from it in the laft. When the *ftone* was colder, thefe appearances were confiderably ftronger; but, on cooling ftill more, they were lefs and lefs.

Exp. 2. I repeated this laft experiment, with the difference only of having the balls electrified *minus* inftead of *plus* *. And in this cafe (as might be expected) the effects were reverfed; the balls *acceding* towards the *convex* fide, and *receding* from the *plain* fide.

Thefe appearances in the two experiments agreeing with one another, fufficiently prove, that *when the ftone has been heated by water, one fide is electrified* minus, *and the other fide is electrified* plus; which is the firft law laid down by *Æpinus*. This ftate has not improperly been called its *natural ftate*; becaufe the *heat*, which difpofed the *Tourmalin* to afford thefe appearances, was *uniform* in every part of its furface, and the *water* itfelf an *uniform* conductor.

Exp. 3. I prefented the *convex* fide to the flame of a candle, but not fo near as to touch the flame, and held it there about one minute, 'during which time the ftone acquired a *plus* electricity on both fides, for

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* I did the fame thing in every experiment where there was occafion to ufe thefe balls, to be more certain of the conclufions. •

the

the balls *receded* from them, but rather with a greater force from the *convex* side, than from the *plain* side.

This appearance proves an *increase of the power in the stone*; because it continued to act for a time like other bodies electrified *plus*. And in regard to the different forces of the two sides, that will be particularly considered hereafter.

Exp. 4. After a short time, the *Tourmalin* being colder, and the remaining heat more equally diffused, it changed its last state to a *plus* and *minus* one; for the *plain* side made the balls *accede*, and the *convex* side made them *recede*.

This change seemed to arise from some alteration, on or near its surfaces, by having the heat equally diffused throughout the stone: if it was not so, I see no reason but that the *Tourmalin* should continue to be electrified *plus* on both sides, whilst any electrical signs remained.

Exp. 5. I now held the *plain* side as near the flame as I had done the *convex* one; and instead of both sides being electrified *plus*, they were electrified *minus*, for each side caused the balls to *accede*.

From this experiment, I had reason to believe that the *stone* was *emptied* (if I may so say) of its electricity; because it continued to act for a time like other bodies electrified *minus*.

Exp. 6. After the same length of time as in the fourth experiment, the *Tourmalin* being colder, it changed its state also; for the *convex* side made the balls *recede*, and the *plain* side continued to make them *accede*, as they had done before.

This

This change feemed likewife to arife from fome alteration on the furfaces of the *Tourmalin*, by having the heat equally diffufed throughout the ftone : if it was not fo, I fee no reafon but that the *Tourmalin* fhould continue to be electrified *minus* on both fides, whilft any electrical figns remained.

I was naturally led next to examine at which furface of the *Tourmalin* the fluid entered (if any did), whilft it continued heating.

Exp. 7. *Flame* being improper for my purpofe, becaufe the electric fluid is readily diffipated by its prefence, I made choice of an iron rod, at the end of which was a round knob. This was heated, and afterwards brought to a certain diftance from the balls, in order to fee if they were moved by it: but not perceiving the leaft motion, I interpofed the *Tourmalin*, with the *convex* fide next the balls. They *acceded* a little, and when I removed the heated iron, they returned to their place again. I then brought the iron nearer to the *Tourmalin* than before : the balls in this cafe *moved* with vigour towards the ftone, and continued in *contact* with it for a confiderable time ; and, after that, they *receded* from it.

On examining the balls, I found they had loft all their *plus* electricity, and were electrified *minus*. I alfo obferved that the *ftone* itfelf was *minus on both fides*.

I gathered from this experiment, that the *electric fluid flowed from the balls towards the ftone* ; becaufe they not only loft their own *plus* electricity, but were electrified *minus :* and, as the *Tourmalin* was *minus* alfo on both fides, *a quantity of electric fluid muft*

have

have flowed from it towards the iron. That this was really the fact will appear prefently.

Exp. 8. When I heated the *convex* fide in the fame manner as I did the *plain* one, the balls were not moved towards the *Tourmalin*, but from it, and continued in that ftate. In this experiment they were electrified *plus*, and the ftone alfo was *plus* on both fides.

I gathered from this experiment that the *plus* electricity in the ftone did not flow from the balls, becaufe they loft none of their virtue; but it muft be *from the iron itfelf.*

It was now neceffary to examine the iron: but I found it very difficult to do this for many reafons, and fuch as might perhaps be thought too tedious if I mentioned them.

I therefore had recourfe to another expedient, where there was a probability of meeting with better fuccefs. This was to make ufe of a tube of glafs about two feet long, one end of which I heated red hot and tried the experiments again: obferving at the fame time the ftate of the glafs after each experiment.

Exp. 9. When the *plain* fide of the *Tourmalin* was expofed to the heated end of the glafs, in like manner as it was to the knob of iron in the feventh experiment, I obferved that about *three inches* of the heated part of the glafs was *electrified minus*, and beyond that diftance it was *electrified plus*, and continued fo even when the glafs was very near cold.

This *plus* and *minus* ftate in the glafs, muft be caufed from the action of the fluid flowing from the

balls and *Tourmalin* towards the glafs : becaufe I found that a current of the electric fluid acting againft the natural quantity of the fluid in the glafs will produce the fame effect : for having rubbed a tube of glafs, I applied it to the heated end of another tube, and the appearances were exactly alike ; that part which was heated being *minus*, and the part beyond it *plus*.

This feems a leading ftep, towards difcovering the truth we are in fearch after : for the current of the fluid feems to be fairly traced in thefe circumftances, the heat having difpofed the *Tourmalin* properly to let the fluid pafs *from the balls, through its fubftance, towards the glafs.*

Exp. 10. I was now eager to try what would be the event, when the heated part of the glafs was next the *convex* fide of the *Tourmalin.* Upon making the experiment, I found that the tube was electrified *minus above one foot in length*; without the leaft appearance of a *plus* electricity beyond the *minus* one, as in t'le laft experiment. And this *minus* appearance continued alfo when the tube was nearly cold.

Now becaufe the *Tourmalin* was *plus*, the balls *plus* alfo, and the heated glafs *minus*, the electric fluid *muft have flowed from the glafs to produce a plus electricity in the ftone and balls.*

Thus you fee, Sir, I have difcovered *two currents of the electric fluid* paffing in contrary directions, the one of them *electrifying both fides of the Tourmalin plus*, and *the other both fides minus.*

MY next ftep was to put the *Tourmalin* into its natural ftate (as *Æpinus* calls it,) in order to make farther experiments.

Exp. 11.

Exp. 11. To anfwer this purpofe beſt, I ſeparated the *Tourmalin* from the wax, and placed it in boiling water for a ſhort time, where it was ſurrounded on all ſides with a conductor and an *equal degree of heat :* then, taking it out of the water, I laid the *convex* ſide (after it was dry) upon the ſlip of wood, ſupported by wax, to which the pith balls were ſuſpended ; but no appearance happened, for the balls continued at reſt.

Exp. 12. But when the *ſtone* remained on the wood a little time longer, the balls ſeparated to a conſiderable diſtance, ſometimes near two inches ; and remained ſo for more than one minute.

In this ſtate they were electrified *plus*, as appeared by their acceding towards amber when it was rubbed and brought near them. From the balls being electrified *plus*, the flow of the fluid from the *plus* ſide of the *Tourmalin* muſt have cauſed ſome part of the wood itſelf to be *minus*, and the balls *plus* ; becauſe the ſame effects happened in *Exp.* 9. Beſides, electrified glaſs oppoſed to the ſame wood produced ſimilar appearances.

Exp. 13. If, whilſt the ſtone continued reſting upon the wood, I brought my *finger* near the *plain* ſide of the *Tourmalin*, the balls *receded farther* from each other ; and when I repeated the approach, it every time affected the balls and made them recede a little more, unleſs the *Tourmalin* was become too cold ; in which caſe they approached nearer, but ſtill continued to be electrified *plus.*

In this experiment, the *finger* did nothing more than ſupply the ſtone with the electric fluid, more readily than the air itſelf would have done.

Exp. 14.

Exp. 14. Upon removing the ſtone *by degrees* from off the wood, the balls approached nearer and nearer : but when it was taken away entirely they receded again, and in this caſe were electrified *minus* inſtead of *pus.*

This is a farther confirmation that the fluid flowing from the *ſtone* electrified the wood *minus,* by forcing part of the natural quantity in the wood into the balls, and ſo made them *plus* ; but becauſe ſome part of the fluid was forced likewiſe out of the balls into the air whilſt they continued *plus,* therefore when the *ſtone* was taken away, they were juſt as much electrified *minus,* as the force of the fluid flowing from the *ſtone* was able to drive out of the balls : and I have formerly ſhewn * that the balls will *recede* from one another in this *minus* ſtate as they do in a *plus* one, by the crouding in of the fluid from the air &c. on all ſides to reſtore the *æquilibrium* ; but being retarded in ſome degree in endeavouring to enter the balls by the *medium* on their ſurfaces, an accumulation is formed, and conſequently atmoſpheres ſimilar to the *plus* ones ; with this difference only, that one is tending from the body and the other to the body.

Exp. 15. After heating the ſtone again in boiling water, I laid the plain ſide upon the wood. In this caſe the balls continued at reſt as they did in the eleventh experiment, when the *convex* ſide laid upon the wood.

* See exp. and obſ. by *Hoadly* and *Wilſon.*

Exp. 16.

Exp. 16. But, after a little time, the balls separated an inch or more, and remained so for some time. In this state they were electrified *minus*; for they *receded* from the amber.

The *Tourmalin*, in these circumstances, was supplied with the electric fluid from the wood and balls, as appears from the preceding experiments, when the heated glass was applied ; so that the balls must have been in a *minus* state, and some part of the wood in a *plus* one : because of the like resistance at the surface to be overcome, where an accumulation of the fluid must have been caused before it could pass to the *Tourmalin* ; as appears by Lord *Charles Cavendish*'s experiment.

Glass electrified *minus*, and applied in the same manner, produced the like effect. As to the method of electrifying glass *minus*, it will be shewn presently.

Exp. 17. Upon bringing my *finger* near the *convex* side, the balls *receded farther from each other*, as they did in the thirteenth experiment : and, on repeating the approach, the balls receded a little more, unless the *stone* was become too cold.

The approach of the *finger*, therefore, conducted the fluid from the *stone*, more readily than the air surrounding it.

Exp. 18. On removing the *Tourmalin* the least from the wood; the balls approached nearer each other, and continued to do so as the *stone* was removed farther off; nevertheless they were electrified *minus*, though in a less degree.

Exp. 19. I then removed it intirely, and the balls *receded*, but to a *greater distance than at any time before*

before in the preceding experiments; and, inftead of being *minus*, they were now *plus*; for the amber caufed them to *accede.*

By the balls *receding* to a greater diftance in this laft cafe than in any other experiment I have yet produced, and the flow of the fluid, during the *natural flate* of the ftone, being from the *minus* to the *plus* fide, *we have produced another proof, that the refiftance is leaft at the minus fide of the ftone :* and from this caufe, the tendency of the fluid from the balls towards the *ftone* muft be greater, than when the fluid tended from the *ftone* towards the balls. But there is a refiftance appertaining to the wood, that was obferved before, and which muft be taken into the account, though it is the fame in each experiment. Neverthelefs, fince the tendency of the fluid is different when different fides of the *ftone* are expofed, different degrees of the electric fluid will be accumulated. Hence we fee the reafon why, upon removing the *ftone* from off the wood, as in this laft experiment, *more* of the accumulated fluid muft have *flowed* in, than *flowed out,* becaufe the balls were electrified *plus.*

HAVING endeavoured to explain thefe *three different flates* of the *Tourmalin,* caufed by different applications of *heat,* I fhall now offer fome farther experiments in which *friction* is concerned, and compare them with other experiments of the vitreous and refinous kind, in order to obferve how far they agree with one another, and whether the principles here advanced are conftant and uniform.

Exp. 20. The *Tourmalin* being again fixed to the wax, I gave the *convex* fide one flight rub with my
<div align="right">*finger,*</div>

finger, and obferved, that both fides were electrified *plus*.

When the *Tourmalin* was put into its *natural ftate*, fo as to electrify *plus* and *minus*, I gave the fame fide another flight rub; and in this cafe both fides were electrified *plus*.

Upon repeating thefe two laft experiments with the *plain* fide inftead of the other, the *Tourmalin* was electrified *plus* on both fides likewife; but with this difference, that now they were confiderably more electrified than before.

This is a farther argument, that *the refiftance is lefs on the plain fide than on the convex fide*; and that the *fluid paffed through the ftone.*

And becaufe fo *flight a friction* occafioned fuch a fenfible alteration, we are fufficiently cautioned from *touching* the ftone at any time, but when the experiment requires it. The fame caution is to be obferved with glafs, amber, filk, &c.

Thefe experiments put me upon trying the experiment with the pane of glafs, mentioned in the beginning of this letter; and, upon finding that the *electric fluid* not only *paffed through the glafs*, but electrified it *plus*, I had a mind to try, whether I could not electrify it *minus*.

Exp. 21. For this purpofe I made ufe of the fame glafs, and, when it was a little warmed, I held it within two feet of the prime conductor, which was electrified *plus*. By this method, that part of the glafs, which was oppofed to the conductor, became electrified *minus on both fides*; but beyond that, a confiderable part all round the *minus*, was electrified *plus on both fides*. This effect is of the fame kind with that

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that mentioned in the 9th experiment. In a few minutes, the *minus* electricity difappeared, and the *plus* continuing, diffufed itfelf into the place of the other ; fo that now the *whole* was electrified *plus*.

Exp. 22. The experiment fo far fucceeding, induced me to make ufe of a lefs piece of glafs, that I might have the *whole electrified minus*. Upon making the experiment, it anfwered accordingly.

Thefe advances led me to obferve the power of electrifying this fmall piece of glafs and the *Tourmalin* at *different diftances*.

Exp. 23. I expofed the fmall piece of glafs to the prime conductor, at the diftance of two feet, which was the fame as before in the 21ft experiment, and obferved a *minus electricity at both furfaces*.

Exp. 24. As I moved the glafs *nearer*, *to a certain diftance*, it was more fenfibly electrified *minus* ; and after that, on moving it ftill nearer, the *minus* appearance was lefs and lefs fenfible, till it came within the diftance of about *one inch*, and then it was electrified *plus on both fides*.

Thefe laft experiments are farther confirmations of the *permeability of glafs*.

Exp. 25. This *plus* electricity in the glafs I found might be changed to a *minus* once again, by removing the glafs, and holding *it for a time at a greater diftance:* which is another proof of the *repulfive power of this fluid*.

Exp. 26. The *Tourmalin* afforded like appearances when in the fame circumftances ; with this difference, that they were caufed at *greater* diftances than thofe of the glafs; and particularly the *plus* electricity was acquired at the diftance of *one foot*, or more.

From

From this difference in the power at different diftances, I inferred, that the *Tourmalin refifts the exit and entrance of the fluid confiderably lefs than glafs, or even amber*; for the diftances requifite to caufe changes in thefe, were lefs, than the diftances which caufed changes in the *Tourmalin :* and fince, by expofing *glafs*, and the *Tourmalin*, to an electrified body, at confiderable diftances, they are rendered *minus*, and at a nearer diftance *plus*; it is no inconfiderable argument that their *general laws are the fame*; and that *the Tourmalin differs in nothing from other electric bodies, but in acquiring an electricity by heat.* And, in regard to this remarkable effect, the experiments I formerly made, which rendered electrics non-electric, as likewife Mr. *Delaval's* curious experiments upon *earthy* fubftances, are other inftances how particular bodies may be fo altered, as to fuffer the electric fluid to pafs through them or not, according to the different degrees of *heat* employed in the experiment.

But to proceed with our obfervations on rubbed glafs.

Exp. 27. Having by me a pane of glafs, one fide of which was rough, and the other fmooth, I rubbed it flightly on the rough fide; upon doing which, *both fides were electrified minus.*

Exp. 28. I treated the other fide in the fame manner; after which, the *minus* electricity was changed to a *plus one, on both fides.*

Now, becaufe the fame glafs afforded different appearances when different fides thereof were rubbed, and there being no other difference in the circumftances of the two experiments, except that in the furfaces themfelves, the one being rough, and the

other fmooth; it follows, that the power of electri-
fying *plus* or *minus arifes from one and the fame fluid.*

Exp. 29. I then had a curiofity to try, whether I
could not, by rubbing, make one fide of this glafs
plus, and the other *minus, at the fame time.* This I
effected, after both fides were made *plus* ; for, by rub-
bing the rough fide of the glafs lefs, than I rubbed
the fmooth fide, that became *minus,* and the fmooth
fide continued *plus.* I rubbed the rough fide *lefs,*
becaufe I found from experience, that *rough glafs*
required a lefs power to electrify it *minus,* than
fmooth glafs did to electrify it *plus* ; and therefore I
concluded, that the *medium on the different furfaces*
has *different powers, the greateft belonging to the
fmooth, and the leaft to the rough furfaces* ; as Sir
Ifaac Newton has fhewn concerning *light* falling up-
on polifhed and rough glafs.

I remember an obfervation of the like kind, which
Mr. Short made, upon having occafion to heat one
of his *metal fpeculums,* behind which was faftened a
wooden handle. This *fpeculum* he placed near a
ftrong fire, with the polifhed furface towards the fame,
where it continued above an hour, *without receiving
the leaft degree of warmth.* That power therefore,
which *reflected the heat,* muft certainly be of the fame
nature with that, which occafioned the *knobs* of light
in *vacuo,* mentioned in the firft experiment. I have
frequently endeavoured to caufe alterations in that
power, with a view to be better acquainted with its
laws ; and, amongft other attempts, I rubbed elec-
trics againft electrics.

Exp. 30. The firft trial was with the *Tourmalin*
and amber, which produced a *plus* electricity on both

2   fides

fides of the *ſtone*, and a *minus* one in the amber. Afterwards I electrified the amber, and held it near the *Tourmalin*; ſtill both fides were *plus* : and if I rubbed the *Tourmalin* whilſt the amber was electrified, it continued *plus*. Then I rubbed the *ſtone* with glaſs ; notwithſtanding which, both fides of the *Tourmalin* were *plus*, and the glaſs *minus*.

Exp. 31. But when the glaſs was electrified *plus*, and held near the *Tourmalin*, as I had done before with the amber, in this caſe, both fides were electrified *minus*.

Theſe experiments ſeem to ſhew, that *where electric appearances are produced, by the rubbing of any two poliſhed bodies together*, that body, whoſe ſubſtance is *hardeſt*, and *electric power ſtrongeſt, will be always plus*, and the *ſofteſt* and *weakeſt*, always *minus*. It was from this theory, with which I had the pleaſure to acquaint you, that I was deſirous of trying to electrify the *Tourmalin minus* by *rubbing*, not having at that time been able to do it. I fixed upon a *brilliant diamond* for this purpoſe, as being the *hardeſt* body, and *ſtrongeſt electric*, that I was acquainted with ; and, upon rubbing the *Tourmalin* with it, my expectations were anſwered ; for both fides of the *Tourmalin* were electrified *minus*, and the diamond *plus*.

Exp. 32. Theſe experiments ſucceeding, I rubbed glaſs againſt glaſs, and found, that *they electrified each other* ; but one of them was *plus*, and the other *minus*.

Exp. 33. Two pieces of amber, treated in the ſame manner, were alſo electrified *plus* and *minus*.

Exp. 34. When I rubbed glaſs againſt amber, the former was *plus*, and the latter *minus*.

U u 2　　　　　　*Exp.*

Exp. 35. Two *Tourmalins* being rubbed againſt each other, one became *plus*, and the other *minus*.

From all which it appears, that ſome alteration was made in the *medium* on their ſurfaces; otherwiſe theſe oppoſite effects could not have been produced: and in regard to the ſame bodies producing different effects, it is not improbable but they may differ in degrees of hardneſs, poliſh, or of their electrifying power.

Now, as *electrics, rubbed againſt electrics, occaſioned electrical appearances*, I was encouraged to try what would be the effect of *air*, if I *rubbed*, or rather *forced*, it againſt electrics; for I ſuppoſed the particles of air to be ſurrounded with a *medium* of the ſame kind as groſſer bodies, which is the cauſe of their being ſo elaſtic.

Exp. 36. To do this, I only made uſe of a common pair of *bellows*, and having brought the *Tourmalin* near to the end of the pipe, I found, after it had received about *twenty blaſts*, it *was electrified plus on both ſides*.

Air therefore ſeems to be *leſs electric than* the *Tourmalin.*

Exp. 37. Into the place of the *Tourmalin* I brought a pane of glaſs, and blew againſt it the ſame number of times as in the former experiment. When I examined both ſides, they were electrified *plus* alſo, but leſs than the *Tourmalin.*

Exp. 38. Amber, treated in the ſame manner, was electrified leſs than the glaſs.

Exp. 39. I had recourſe next to a ſmith's bellows. The difference theſe occaſioned was only a much
ſtronger

ſtronger electricity in the *Tourmalin.* Amber was ſtill weaker than the glaſs, and the glaſs weaker than the *Tourmalin.*

Still having in view the *medium* on the ſurfaces of the particles of air, I conſidered, that *heat* would rarify it; by which means, air, having its reſiſtance leſſened, would more readily part with the electric fluid, and of conſequence electrify more powerfully.

Exp. 40. The *pipe* of the *bellows* being made red-hot, I blew againſt the Tourmalin twelve times only, which was eight blaſts leſs than in the former experiments with cold air. In this experiment likewiſe the *Tourmalin* was electrified *plus* on both ſides, but to a conſiderable degree more, than was done in the 36th and 39th experiments. The hot air had the ſame effect upon glaſs; but electrified it leſs than the *Tourmalin :* and amber, though, like the other bodies, it ſuffered an increaſe of power by the ſame treatment, was electrified the leaſt of all.

From the air electrifying more powerfully when it is hot, than when cold; and the Tourmalin being electrified more than glaſs, and glaſs more than amber, as appears by theſe laſt experiments, we ſeem to have obtained a proof, that the whole atmoſphere is conſtantly promoting a flow of the electric fluid, by the alternate changes of heat and cold : and further, that air is not only leſs electric than the *Tourmalin,* but leſs than glaſs, and even amber.

Exp. 41. When the *Tourmalin* had received the ſame number of blaſts againſt the *plain* ſide, whilſt my *finger* touched the *convex* ſide, it afforded different appearances; for the plain ſide was electrified *plus,* and the other *minus.* After a ſhort time, *both*

fides were plus; and fome time after this the *ftone* recovered its *natural ftate*, the *plain* fide being *minus*, and the *convex plus*.

What appeared fingular in this experiment, was, that the *middle ftate of the ftone* fhould be *plus on both fides*. But we no more wonder at this, when we confider, that there were two caufes to produce thefe effects: the firft was *heat*, which put the *ftone* into an *unnatural ftate* (as *Æpinus* had obferved before) ; for, upon cooling, it would recover its *natural ftate*, and confequently afford different appearances.

But the fecond caufe, which electrified both fides *plus* when the *ftone* was in an intermediate (or, as *Æpinus* calls it, *neutral*) ftate, between the two extremes, was, that then the effects of the air itfelf took place, and electrified *both fides plus*, as it had done before in the 35th experiment.

Exp. 42. The convex fide was now prefented to the *bellows* in the fame manner, and received an equal number of blafts. In this experiment *both fides were plus*, but weaker than in the laft experiment; and, after a time, the *ftone* returned to its *natural ftate*, affording a *plus* and *minus* appearance.

This property in the air, of electrifying glafs, amber, &c. will, in all probability, account for an experiment I met with in *France*, made by *M. Le Monnier* *, who fhewed me, at *St. Germains en Lays*, two or three long wires, which he had fufpended horizontally from the palace to his apartment, above

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* See the Memoirs of the Academy of Sciences.

thirty

thirty feet from the ground (the fame experiment that was made by my friend the *Abbé Mazeas*, and communicated, with feveral others, to the Royal Society *), in order to obferve the *electric effects during thunder ftorms and cloudy weather*. Thefe *wires* he frequently *found electrified in a fmall degree, when the day was clear*, and *without the leaft appearance of a cloud*. Might not this effect therefore arife from *the friction of the air againft the wires*?

From confidering all thefe things, and what thefe laft experiments have taught us concerning the different effects produced by *hot and cold air*, it feems probable, that many of the curious operations in nature arife from a *conftant flux and reflux of the electric fluid*. And if the obfervation be true, that air, free from moifture, during tempefts and hurricanes in the night, frequently affords a faint kind of light, refembling what is feen in an exhaufted receiver, through which the electric fluid is caufed to flow, as alfo it occafions a more general clearnefs in the heavens, than would appear were there no fuch violent agitations ; it is reafonable to imagine, that *a flux of the electric fluid in the air*, is the caufe of fuch appearances ; for the fame reafon that *our artificial blafts* produced the electrical effects I have juft now mentioned.

· But it remains to be inquired into, whether the *Tourmalin* is fo difpofed by nature, as to fuffer the electric fluid to pafs through it only in one direction, like *magnetifm* through the *loadftone*, or is indifferent which way it flows.

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* See the Tranfactions of the Royal Society, Vol. XLVIII.

Exp.

Exp. 43. Upon examining a *Tourmalin* which was *flat on both sides*, and *polished*, except upon its edges, part of the edge appeared *plus*, and another part oppofite to it *minus*; fo that a line, drawn from the *plus* part through the centre of the ftone to the other fide, would pafs through the *minus* part.

Exp. 44. Two fmaller *Tourmalins*, that were flat alfo, and polifhed, like the other, exhibited the fame appearances.

Exp. 45. Another *Tourmalin*, which was alfo flat, but *unpolished*, afforded a fourth inftance of this kind.

Exp. 46. The firft of thefe *Tourmalins* was afterwards *polished*, as well at the edges as the furface; in order to fee, whether that would make any alteration; but I found it ftill retained its former electrical ftate.

Exp. 47. I experienced the fame with another *Tourmalin*, which had been *rough* likewife.

Exp. 48. I then, with a little emery, made that edge, which was *plus, rough again*, preferving all the reft *fmooth*; but I could not perceive, that any alteration was made by it. I did the like with the edge of the other polifhed *Tourmalin*, without being able to obferve any difference.

Exp. 49. As to the fmall *Tourmalin*, that is *plain* on one fide, and a little *convex* on the other, which you obferved to be a very good one, it is *plus* on the *plain* fide, and *minus* on the *convex*; which is contrary to the large *Tourmalin* defcribed in the beginning of this letter.

Exp. 50. I had an opportunity of trying another *Tourmalin*, now in the *British Mufeum*, which afforded another inftance of the fingular difpofition of
this

this *ſtone*. The *Tourmalin* I now ſpeak of, is *plain* on one ſide, and *concave* on the other; with a ſmall flat border round it. It was ſquare, but one of the corners had been broken off. When I examined it, the broken part was *plus*, and the corner oppoſite to the broken part *minus:* ſo that here alſo the electric current ran through the ſtone in a diagonal line.

Exp. 51. Every one of theſe *Tourmalins*, except that in the *Britiſh Muſeum*, I greaſed all over, and, whilſt they were warm enough to preſerve the greaſe liquid, I tried each *Tourmalin* ſeparately, but found, no alteration in the virtue of the ſtone, except weakening it a little; though it is well known, that moiſture, of any ſort, readily conducts the electric fluid; and therefore, if the *Tourmalin* had not a fixed kind of electricity, the *plus* and *minus* obſerveable on the two ſides of the ſtone, muſt, by this treatment, have united, and deſtroyed each other: the *plus* ſide parting with as much of the fluid, as the *minus*, on the other ſide, wanted, to reſtore the *equlibrium*.

Upon the whole, all theſe experiments do moſt clearly prove, that the *Tourmalin ſuffers the electrical fluid to paſs through it only in one direction, and ſo far it bears ſome analogy to the loadſtone.* And as the *loadſtone loſes its virtue by being made red-hot,* I was deſirous to ſee what would be the event in the *Tourmalin under the ſame treatment.*

Exp. 52. I therefore put one of the flat Tourmalins into a ſtrong fire, for half an hour; but could *not,* afterwards, perceive *the leaſt alteration.* I made the ſame experiment upon another *Tourmalin,* with the ſame ſucceſs.

Exp. 53. Laftly, I heated the ftone again, and, whilft it was red-hot, I threw it into water; by which treatment the *virtue of the Tourmalin was intirely deftroyed*, and it had the appearance of being fhivered in many parts, without breaking.

IN regard to the internal frame of the *Tourmalin*, we can fay nothing; yet fo much I have learned, by thefe experiments, that there are *three different methods of heating* the *Tourmalin*, which produce different electric appearances; that *different degrees of heat*, afford different appearances; that *friction* has the fame effect upon it, as upon glafs; and that the *Tourmalin*, when it is heated properly, fuffers a current of the electric fluid to pafs through it *in one direction only :* fo that the *Tourmalin* hath, as it were, *two electrical poles*, which are not eafy to be deftroyed or altered; and farther, *that there is not any fubftance in nature, which we are acquainted with, that the electric fluid does not readily pafs through*; that there feems to be *a conftant flux and reflux of it in all bodies, as well in the air as in* vacuo, occafioned by the alternate changes of *heat and cold* in every part of this globe. All which things do very much confirm the opinion I formerly entertained, and attempted to prove by experiment, that the *electric fluid is diffufed throughout the whole earth*, as well as in *the air* furrounding it *.

How far this flowing of the electric fluid may be concerned in the ordinary operations of *nature*, by keeping up that motion, which feems fo neceffary in the feveral parts of the *grand machine*, I leave to others, who may be more fuccefsful in their refearches.

* See a Treatife on Electricity, the 2d Edit. by B. Wilfon.

By

By thefe advances, we have likewife attained to a more certain knowlege of that *medium*, which feems to *furround the furfaces of all bodies*, and which, being of a greater or lefs denfity, or exercifing (if I may fo fay) a greater or lefs refiftance, produces different effects, as the *electric fluid* (or *æther*, if you pleafe) *paffes in* and *out of bodies*, whenever they are difturbed by any external violence; and, were it not for this *medium furrounding all bodies*, the electric fluid, I apprehend, could *neither be accumulated, nor detained, in any body* whatfoever.

This principle is very fimple, and feems to be of a very general nature. Our great *philofopher* adopted it from many experiments: he fuppofed, that the wonderful phænomena of *nature*, particularly thofe of *light*, were not to be explained without it; and therefore did not fcruple to propofe it, as a principle, to be farther inquired into.

I fhall think my time well employed, if, by thefe inquiries, I have at all contributed to fhew, how much we arc indebted to this happy *Interpreter* of *Nature*, and have afforded frefh occafion, by the light of his labours, to admire, and adore, the *firft caufe of all things.* But whatever may be the fuccefs of my endeavours, they have at leaft been attended with a fatisfaction, which not a little increafes that defire I have ever had for purfuits of this kind. I am,

S I R,

Your moft obliged humble fervant,

Great Queen-ftreet, London,   .   Benja. Wilfon.
   Nov. 9th, 1759.

XXXVI. *New Experiments and Observations concerning Electricity* ; *by* Robert Symmer, *Esq*; *F. R. S.*

PAPER I.

Of the Electricity of the human Body, and the Animal Substances, Silk and Wool.

Read Feb. 1. 1759. I had for some time observed, that upon pulling off my stockings in an evening they frequently made a crackling or snapping noise ; and in the dark I could perceive them to emit sparks of fire. I made no doubt but that this proceeded from a principle of electricity; and I was confirmed in this opinion, by observing that, in weather favourable for electrical experiments, those appearances were more remarkable than at other times. I mentioned this observation to several of my friends, and some of them told me, they likewise had often perceived the snapping, and the emission of fire from their stockings upon pulling them off, especially in the winter evenings: but I could not hear of any body that had taken this phenomenon into consideration in a philosophical way. For my own part, I could not but think that so striking an appearance, one that seems to have an immediate connexion with the human body, or is at least as much about us as the cloaths we wear, and is obvious to the perception of our senses, merited not only a philosophical attention, but the strictest inquiry possible. I was the more in-

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duced to fuch an inquiry, as it feemed to me to open a new path for proceeding in electrical refearches, and might perhaps throw new light on the great and furprifing difcoveries already made in that branch of natural philofophy. The fimplicity of the appara-tus, and the great facility in making the proper ex-periments, putting it in my power to begin and carry on my enquiry at pleafure, I entered upon fomewhat of a regular courfe of obfervations about the begin-ning of November laft; and fince that time have purfued it as clofely as my leifure, and the weather, which has been far from favourable for electrical ex-periments, would permit. It is the purpofe of this, and of thofe papers that may follow, to lay before the Royal Society an account of the experiments and obfervations I have already made, or may hereafter be able to make, in the progrefs of this inquiry.

My firft endeavour was to difcover what fort of ftockings was moft proper to produce electricity. In order to determine this, I tried fingle ftockings of different kinds, *viz.* thread, cotton, filk, and worfted, putting them on, and wearing them fome time. On pulling them off, I could perceive nothing of elec-tricity in the thread or cotton, and no remarkable de-gree of it in the filk and worfted.

When I fay *no remarkable degree of it*, it may be proper to be a little more explicit. I muft therefore obferve, that filk and worfted, being in themfelves electric, are both of them, efpecially filk, extremely fufceptible of electricity. I have fometimes obferved, particularly when the weather was favourable, that filk, when but barely handled, nay, when but fim-ply touched, has become electrical where it had been

handled.

handled or touched, as appeared by its attracting little light balls suspended by threads. It cannot therefore be supposed that silk or worsted stockings can be put upon the leg, without being excited to a sensible degree of electricity. And thence it is, that when taken off they are sometimes perceived to snap, though worn single. But whatever electricity the single stocking acquires by friction or otherwise, it immediately loses upon being separated from the leg: if any electric virtue remains, it is no more than what belongs to it as an electric substance ceasing to be excited; and it is in so small a degree, as in the present case not to merit attention. In general, when I speak of the electricity in question, I mean such a power of electricity as is obvious, and perceptible to the senses; so that the stocking, after being taken off, should appear more or less inflated; throw out an electrical wind to be felt by the bare leg; attract or repel another stocking visibly; and, upon the touch, snap, and emit, or receive electrical fire.

After making the experiment above-mentioned with single stockings, I proceeded to try the effect of two stockings upon one leg. This I did with two of thread, cotton, worsted, and silk successively; but this produced no electrical appearance more than before. I then combined them one with another, and, running through all the different changes, I found that none of those I then made use of exhibited visible proofs of electricity, but the silk and worsted together; and there, indeed, the electric power appeared remarkably strong. It seemed to be a matter of indifference whether the silk or the worsted was uppermost, the combination of the two was what I
judged

judged to be effential; and happening at that time to wear filk ftockings with thin worfted under them, I kept to the ufe of thefe, and found it a convenient circumftance in the courfe of my experiments.

As fome may have the curiofity to examine the obfervations I prefent them with, by experiments of their own making; before I proceed further, I fhall furnifh them with a few remarks, which may enable them to make their experiments with more eafe and certainty, than if they fet out unacquainted with fome circumftances, which I have learnt from experience.

. One of the firft things to be attended to, is the weather, which has an influence on all electrical experiments, but upon none more than thofe which relate to this branch of electricity. The moft favourable weather is that which is dry and clear, and, if a little frofty, fo much the better. In general, when the quickfilver in the barometer rifes, and the fire burns remarkably brifk and clear, we may expect a confiderable appearance of electricity: at other times, it is better not to attempt the experiment.

▪ When the weather is favourable, it is not neceffary to wear the ftockings all day: That, indeed, raifes the electrical power to the higheft degree ; yet, provided they be dry, and made warm by the fire, before they are put upon the leg, their continuance upon it for a very fhort time, prepares them fufficiently for exhibiting vifible proofs of electricity. If this fhould not anfwer upon the firft trial, the operation may be repeated two or three times, to bring them into a proper difpofition to receive the electrical virtue; and this is no more than what is found to be fometimes

neceffary

neceffary with refpect to glafs globes and tubes, efpe-
cially when new. If the ftockings have once acquired
this difpofition, they retain it for the day, or until a
change of weather, and the experiment may be re-
peated with fuccefs as often as one pleafes ; for no
fooner are they difcharged of their electricity, than
they are ready to receive it anew ; nay, if fpeedily
returned upon the leg, they catch it inftantaneoufly,
and may immediately be pulled off to exhibit a new
explofion : what is ftill more furprifing, they feem
to gather force by the frequency of the repetition,
and, to fome certain degree, increafe in electrical
power, provided they be all along kept warm and
dry, and that the leg continue warm.

Another circumftance to be carefully attended to,
is the manner in which they are to be taken off; for
as to the putting of them on, it is a matter of indif-
ference how that is performed. In taking them off,
care muft be had not to feparate them ; for if that
happens in pulling them off, all the electricity efcapes.
The beft way, is to put the hand between the leg and
the ftockings, and pufh them off together. Nothing
more remains to be done, than to pull them afunder;
for upon that, they both of them exhibit a degree of
electricity, which, when at the higheft, is really fur-
prifing.

Before I conclude thefe occafional remarks, it may
not be improper to obferve, that it is not abfolutely
neceffary that the ftockings be applied to the bare
leg; if a fine thread ftocking feparate them from the
leg, though left upon it when they are taken off, it
does not much impair their electricity. But it is more
convenient to ufe the hand inftead of the leg. The
infertion

infertion of the hand into the ftockings, is alone fuf-
ficient, efpecially in favourable weather, to commu-
nicate fuch an electric power, as renders them capable
of anfwering expectation, in fuch experiments as I
have had occafion to make *.——I now return to my
fubject.

The opinion I had conceived, that the combination
of filk and worfted was neceffary for the production
of electricity, feemed to meet with confirmation from
all the experiments I made with the ftockings I then
wore. I was at that time in mourning, fo that my
filk ftockings were black, and under them I conftant-
ly wore thin white worfted. About the latter end of
November I went out of mourning, and of courfe
changed the colour of my ftockings. On the fecond
of December, having put on a pair of white filk
above the worfted, after I had wore them fome
hours, I refolved to amufe myfelf with a few expe-
riments. The weather was remarkably favourable,
and I had reafon to expect a fine appearance of elec-
tricity : but upon taking off my ftockings, and pull-
ing them afunder, to my great aftonifhment, I found
they difcovered no figns of electrical power; as I

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* An improvement may perhaps be made upon this, by getting
a piece of filk or worfted knit, or wove in the ftocking loom, fo
that without being cut, it may be formed into the fhape of long mit-
tens or fleeves, or rather into what we might call a filk or worfted
tube. The reafon why it ought to be knit, or wove in the ftocking
loom, is, that it may better embrace the hand or arm ; and like-
wife, that it may be more retentive of electric virtue, which would
efcape more eafily from a piece of filk or worfted wove in the com-
mon loom, and cut with a number of ends of threads expofed, than
from any thing formed in the fafhion of chain work, and confifting
of one fingle thread of filk or worfted.

held

held them in my hands they hung down collapſed, and did not more attract one another, than before they were put upon my leg. I repeated the experiment two or three times, but with no better ſucceſs. An event I ſo little expected, diſconcerted me much. I ſaw I was no longer to aſcribe electricity to the combination of ſilk and worſted; but I remained at a loſs to know to what I *ſhould* aſcribe it. At laſt, upon conſidering the circumſtances of this and other experiments, a conjecture occurred, that the electricity in queſtion might depend upon the nature of different colours. In order to determine this, I thought it faireſt to make the trial in the ſame ſubſtances. Accordingly I had recourſe to the following experiment.

I took a pair of white ſilk ſtockings, and having warmed them at the fire put them both upon the ſame leg. After I had worn them about ten minutes, I took them off, and pulled them aſunder, but diſcovered no ſigns of electricity in either. I did the ſame with a pair of black ſilk, but to no other effect. I then proceeded to the deciſive trial. I put a black and a white ſtocking upon my leg, and wore them likewiſe ten minutes. I waited with ſome impatience to ſee the ſucceſs of my experiment, and in return had the ſatisfaction of obſerving, upon their being pulled aſunder, that each of them had acquired a ſtronger degree of electricity than I had before ſeen: they were inflated ſo much, that each of them ſhewed the entire ſhape of the leg, and at the diſtance of a foot and a half they ruſhed to meet each other. I went through the ſame experiment with worſted ſtockings, and found that, as in ſilk, nothing but the combination of black and white produced electricity.

As

As I had often experienced the power of electricity in the combination of black silk with white worsted stockings, there remained to try but that of white silk with black worsted, which answered as I expected, and seemed to complete the demonstration.

A phenomenon so new, and of so curious a nature as this experiment presented, could not but for some time engage my attention. I saw, that if this appearance did not arise from some accidental or collateral cause, correspondent effects would follow, upon the combinations of the intermediate degrees of light and shade, between the extremes of white and black. I have had a particular attention to this in the experiments I have since had occasion to make, and so far as I have yet been able to go, it appears to answer my expectation. Nevertheless, as this is a matter that merits a more minute examination, we may hereafter take it up as an immediate object of inquiry; with a view to determine, if light and colours have, of their own nature, a relation with electricity, and in what that relation consists.

In the mean while I shall pursue my principal design, which is to investigate from experiments the nature of that electric virtue, with which the animal substances under consideration appear to be endued. I have already made some progress, farther than what is contained in this paper; and I shall continue to follow the views that seem naturally to open to my inquiry. If I make any discoveries, or come at any conclusions that may merit the attention of the curious, they shall, together with the experiments on which they depend, be submitted to the judgment of this learned Society.

New

New Experiments and Observations concerning Electricity.

PAPER II.

Of the Electricity of black and white Silk.

Read May 17, 1759. IN my former paper, I gave the Society an account of some experiments, made with silk and worsted stockings; by which it appeared, that the remarkable degree of electricity they had acquired, by being put upon the leg, depended on their being of different colours, namely, black and white. I did not then, nor do I yet, take upon me to determine the cause of this phenomenon. Whether it be owing to light, which is the origin of colours; or only to the ingredients, which enter into the composition of the several dyes; or to those conjunctly with the colours they produce: in any of these views the matter is curious, and equally merits a careful inquiry.—But I fear the solution of this, and of many other difficult questions concerning electricity, will depend upon the establishment of a more perfect theory than we have yet attained to. Till we shall be so fortunate as to discover the nature and properties of the powers employed in these operations, we must be satisfied to pursue the path of experiment and observation, in quest of those, as first principles. This is the method I shall continue to follow; and having already shewn the manner of electrifying the black and the white stocking, it is now my purpose to give an account of the appearances and powers of that electricity so excited.

8   Having

Having made a great number of experiments fince I had the honour of prefenting my former paper to the Society, I have had an opportunity of obferving, that the electricity produced between black and white filk, is ftronger than that between filk and worfted of thofe different colours, and a great deal ftronger than that between worfted and worfted : the laft is fo weak, except in time of froft, or when a fharp north-eaft wind blows, that tho' the effects are always of the fame nature, yet they are fometimes fo languid as to be fcarce perceptible. I fhall therefore, and for the fake of brevity, confine myfelf, in this paper, to what is obferveable with regard to the electricity be-tween black and white filk.

It is proper to mention another circumftance : having found it troublefome to electrify the ftock-ings, by putting them as often on my leg as was re-quifite in making experiments, I have quitted that method intirely ; and fatisfy myfelf with the degree of electricity which is excited in the ftockings by drawing them upon the hand : and this is to be un-derftood with regard to all the experiments and ob-fervations I may have occafion to mention, unlefs when otherwife expreffed. The electricity thus pro-duced is not equally powerful with that which is ex-cited by means of the leg ; but it is neverthelefs fuf-ficiently ftrong to anfwer all the purpofes in view ; and it is attended with this advantage, that the ftockings continue longer fit for thefe experiments : for, like other electrical aparatus's, they muft be kept clean, and free from all extraneous matter ; and are therefore moft to be depended upon when new, or when newly wafhed.

To

To give a diſtinct account of the electricity of black and white ſilk, I ſhall trace it through its whole proceſs, beginning before the ſtockings of the different colours are put together.

After being a little air'd at the fire, when the black ſilk is drawn ſingle upon the hand, a crackling noiſe is heard; and in the dark, ſparks of fire may be perceived as paſſing between the hand and the ſtocking: while it is drawn backwards and forwards the crackling continues, and is moſt conſiderable upon the ſeparation of the ſtocking from the hand. Thus it appears, that black ſilk is highly ſuſceptible of electricity; that it is produced almoſt inſtantaneouſly, or at leaſt with very little friction; that moſt of it eſcapes, while the ſtocking is yet upon the hand; and that, upon the total ſeparation, very little remains. This is ſimilar to what happens with the glaſs tube, when the hand, after paſſing along it in one direction, repaſſes it in the other. But ſtill the electricity that the ſtocking retains, after it is ſeparated from the hand, is conſiderable enough to attract or repel little light bodies at the diſtance of one or two feet: ſome degree of inflation in the ſtocking is likewiſe perceiveable; and when a non-electric is brought near it, a crackling is heard, and in the dark ſparks may be ſeen. If two black ſtockings be drawn upon the hand at a time, the appearances are much the ſame as before; only that the ſtockings, when taken off and ſeparated, give ſmaller proofs of electricity, than if each of them had been ſingle upon the hand.

Having found it neceſſary, as I proceeded in my courſe of experiments, to fix upon ſome method of aſcertaining the principal appearances of electricity;

and marking the degree of it, I had recourfe to the little pocket electrometer of Mr. Canton's contrivance, which is defcribed in Vol. XLVIII. N°. 93. of the Tranfactions of this Society. When this inftrument is fupported by glafs, it not only fhews the attraction and repulfion, in general, of electrified bodies, which is one of the moft effential properties of electricity, but it diftinguifhes between the pofitive and negative ftate of electricity, according to the reciprocal attraction or repulfion of the little balls. By the terms *pofitive* and *negative*, I mean only to denote the oppofition of the two different ftates. The particular allotment of the one or the other term appears to me to be arbitrary; but that I may not differ unneceffarily from others, I fhall apply the word *pofitive* to that ftate, in which a body is found to be, when electrified by the clear glafs tube, rubbed by the hand; and the word *negative*, when electrified by the rough or opaque glafs tube, of Mr. Canton's invention (defcribed in the Tranfaction mentioned above), when rubbed in the fame manner, or by fulphur or wax excited. In other words, when the body is in a ftate of repulfion with the former of thofe tubes, we fay it is *pofitively* electrified, and *negatively* when in a ftate of repulfion with the latter, or with fulphur or wax.

Nothing appears to be more wonderful than this double ftate: here electricity feems to counteract itfelf, the electrified body attracting in the one, what it would repel in the other cafe, and *vice verfâ*. As this remarkable property may be traced, in its confequences, through almoft all electrical appearances, I cannot but think it merits great attention, and, when

it comes to be better underſtood, may throw much light upon the ſyſtem of electricity. However that may be, it was impoſſible for me not to obſerve, that it runs through the whole of that branch, which I have at preſent under examination, and to which I return.

White ſilk differs much in electricity from black ſilk. When the white ſtocking is drawn ſeparately upon the hand, no crackling is heard, nor ſparks of fire ſeen in the dark, let it be pulled backward and forward ever ſo often: when another white ſtocking is drawn on above it, nothing more appears: and, when ſeparated from the hand, neither of them diſcovers any ſigns of electricity, excepting that, when brought within a few inches of the electrometer, they attract and repel the balls a little.

If inſtead of two white or two black ſtockings, one white, and over that, a black ſtocking be drawn upon the hand, they diſcover not the leaſt ſigns of electricity while they continue upon the hand, even tho' they ſhould be drawn backwards and forwards upon it ſeveral times; nor, when taken together from the hand, and preſented to the electrometer, do they appear to have acquired any more than a very ſmall degree of electricity. They muſt be brought within the diſtance of a foot, nay, ſometimes of a few inches, before they have any effect upon the balls: but the moment they are ſeparated, they are found to be both of them highly electrified, the white *poſitively*, and the black *negatively*. The circumſtances, that appear the moſt to merit obſervation, are as follow :

1°.—When the electrometer is placed on a non-electric, and the black stocking is presented to it at the distance of 3, 4, or 5 feet, according as it happens to be more or less powerfully electrified, the balls begin to be visibly attracted, and when it is brought nearer, they are seen to be agitated in a violent manner. If, instead of the black, the white stocking be presented at the same distances, it is found to have precisely the same effects, attracting and agitating the balls in the very same manner: From whence it appears, that whatever difference there was between the electricity of the black and the white, under other circumstances, they each of them acquire an equal degree of electricity, by being electrified together.

2°.—When the electrometer is supported by glass, and the white stocking is presented to it, it first attracts the balls, and afterwards repels them; when taken away, it leaves them in a repulsive state with regard to each other; when brought back, it repels them as before. If, in place of the white, the black be now presented, the balls are immediately attracted, soon after again repelled, and left once more in a repulsive state with regard to each other. If the white be again presented, the same train of effects takes place as before; and so on, alternately, as in the case of the clear and opaque glass tubes, when excited; the white stocking answering precisely to the clear, and the black to the opaque tube, and acting the one *positively*, the other *negatively*, at full as great a distance, and as forcibly, as the tubes.

3°.—Both the stockings, when held at a distance from one another, appear inflated to such a degree,

that, when highly electrified, they give the intire
shape of the leg; and when brought near the face,
or any naked part of the body, there is a senfation
felt, as if a cool wind was blowing upon that part.
When the two white, or the two black, are held to-
gether by the extremities, they repel one another,
and form an angle, feemingly of 30 or 35 degrees.

4º.—When a white and a black ftocking are pre-
fented to each other, they mutually attract, with a
force anfwerable to the degree of electricity they have
acquired : when brought within the diftance of three
feet, they ufually incline towards one another : within
two and a half, or two feet, they catch hold of each
other ; and when brought nearer, they rufh together
with furprifing violence. As they approach, their
inflation gradually fubfides ; and their attraction of
foreign objects diminifhes : when they meet, they
flatten, and join as clofe together, as if they were fo
many folds of filk ; and then the balls of the electro-
meter are not affected at the diftance of a foot, nor
even of a few inches at certain times. But what
appears moft extraordinary, is, that when they are
feparated, and removed at a fufficient diftance from
each other, their electricity does not appear to have
been in the leaft impaired by the fhock they had in
meeting. They are again inflated, again attract, and
repel, and are as ready to rufh together as before *.

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* The phenomena, here remarked, of the black ftocking and
the white when electrified ; namely, that, as they approach one
another, their attractive and repulfive force decreafes, with regard
to foreign objects, but increafes furprifingly, with regard to each
other ; and that their electricity fuffers no diminution from the
fhock

When this experiment is performed with two black ſtockings in one hand, and two white in the other, it exhibits a very curious ſpectacle: The repulſion of thoſe of the ſame colour, and the attraction of thoſe of different colours, throws them into an agitation that is not unentertaining, and makes them catch each at that of its oppoſite colour, at a greater diſtance than one would expect. When allowed to come together, they all unite in one maſs; when ſeparated, they re-ſume their former appearance, and admit of the repe-tition of the experiment as often as you pleaſe; till their electricity, gradually waſting, ſtands in need of being recruited.

5°.—When they are ſeparated from one another, they loſe their power very ſoon, much as the excited tube does; but when they are together, they will re-tain it for an hour or two, and longer, when the air is in a ſtate favourable for electricity. While they are aſunder, and any non-electric is brought near them; if that non-electric is of a broad ſurface, it is with difficulty they are diſcharged of their electri-city; but if the point of any, eſpecially of a metallic, body, be preſented, they are inſtantaneouſly deprived of their electrical virtue: but if they be in conjunc-tion together, they retain their electricity with ſo much obſtinacy, that even the ſharpeſt point of me-tal cannot deprive them of it. In this, and in ſome

shock of their congreſs, appear to me to be obſervations new in electricity, and to merit attention. They ſeem to point out a re-tentive power of electricity, which takes place between electrics and electrics only; and which, I apprehend, may be found to be the cauſe of many curious and ſingular phenomena.

other

other refpects, there appears to be fuch a refemblance
between the Leyden phial, or the electrical pane of
glafs, and the black ftocking in conjunction with the
white, efpecially when the one is within the other,
that I have been induced to confider them both in the
fame light. In both cafes, the *pofitive* electricity is
on the one fide, and the *negative* on the other; and
the ftockings, as well as the phial, and the pane of
glafs, are at once electrified *pofitively* and *negatively*.
In both cafes there is an accumulation of electricity,
and a retention of it, far beyond what is to be met
with in a fimple body, electric or non-electric. There
is, however, a very remarkable difference between
them in two refpects. In the phial, and in the pane
of glafs, an explofion is always obtained by carrying
on a communication between the two fides by the in-
terpofition of a non-electric; but, in the cafe of the
black ftocking and the white, I never yet have been
able to procure an explofion, nor fo much as a fpeedy
difcharge, by any means I could think of, while the
one was within the other. I have put one hand
within the innermoft, and with my other have clafped
the outward ftocking; nay, I have thruft in my
hand, and turned the ftockings infide out, and, in
that condition, have dafhed them againft the floor;
and all this without procuring the leaft perceptible
difcharge. On the other hand, the phial and the
pane of glafs afford no opportunity of feparating the
pofitive from the *negative* electricity, fo as to fhew
them intire and diftinct from each other; whereas
we need only pull the ftockings afunder, and then in
the white we find the *pofitive*, and in the black the
negative electricity.

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6°.—When the ftockings are feparated, and in the dark, upon prefenting to them the point of one's finger, or any fmall metallic body, rounded at the end. they exhibit the appearance of electrical fire or light, according to the *negative* or *pofitive* ftate of the ftocking the object is prefented to. With the black, at the diftance of two or three inches, there appears to dart from the end of one's finger a fprig or pencil, as it were, of fiery fparks, which dilates in its pro-grefs, and ftrikes againft the furface of the ftocking: at the fame time a crackling, or fnapping noife, is heard. When the firft difcharge is made, upon pre-fenting the finger to a frefh part of the ftocking, the fame phænomenon is repeated, till you have traverfed the whole length of the ftocking, which, when the finger moves flowly, ufually yields eight or ten dif-tinct difcharges, before it is divefted of its electricity. With regard to the white flocking, the fame appear-ances hold; but with this difference, that, inftead of fparks of fire iffuing from the finger, a little globule of white or blueifh light is feen at the point of it; and, when the electricity is ftrong, that little body of light feems to break in an explofion between the ftocking and the finger; and rather a hiffing than a crackling noife is heard.

7°.—The electrical phial may be charged by the ftockings, either *pofitively* or *negatively*, according as the wire from the neck of the phial is prefented to the white or the black; and in the one, or the other cafe, the hiffing, or the crackling noife, is louder than when any common wire, or non-electric body, is prefented: but if the electricity of the white ftock-ing be thrown into the phial, and upon that the

elec-

electricity of the black, or *vice verfâ*; in that cafe, the phial will not be electrified at all.

The charging of the phial was among the firft of my experiments with electrified ftockings. By fome trials I made in the month of December laft, I found it would fucceed. One frofty evening in that month, having thrown into a fmall phial, filled with quick-filver, the electricity of one black ftocking, I receiv-ed from the explofion a fmart blow upon my finger. With the electricity of two ftockings, the blow reach-ed both my elbows; and, by the means of four, I kindled fpirits of wine in a tea-fpoon, which I held in my hand, and, at the fame time, I felt the blow from my elbows to my breaft. It may not, however, be improper to obferve, that the electricity, in this cafe, was excited by means of the leg.

From what hath been faid in the preceding pages, it is evident, that all the remarkable appearances of electricity, hitherto difcovered, may be exhibited by a fimple apparatus of black and white filk. But this is not all: in the courfe of experiments above-men-tioned, fomething curious has occurred to my obfer-vation, of which I do not find that any notice hâs been taken by others. — What I mean is, a ftrong cohefion produced by electricity. But as this paper is already extended to a great length, I fhall referve the account of electrical cohefion for another, which I fhall foon have the honour of prefenting to the Society.

New Experiments and Observations concerning Electricity.

PAPER III.

Of Electrical Cohesion.

According to what I promised in the conclusion of my last paper, I come in this to give the Society an account of cohesion produced by electricity. I had not made any great progress, in the series of experiments and observations I have already had the honour to communicate, before I perceived that the black and white flocking, when electrified, and allowed to come together, not only joined extremely close, but actually stuck to each other; infomuch that unless when weak in electricity, or improperly applied, I could make the white bear the weight of the black, or the black that of the white, and that for a considerable time. But as the weather, for some months in winter, was so seldom favourable for electrical experiments, that I had scarcely opportunities sufficient to satisfy myself with regard to other points, I did not enter upon an examination of this phenomenon experimentally, till about the latter end of March. By that time, I had got ready the scale of a balance properly fitted with a hook to catch hold of the stocking, a set of Troy-weights I could depend upon, and an exact pair of scales, to take the precise weight of the stockings as occasion should require.

By

By experience I found that the fame pair of ftockings did not always do equally well, even independently of the weather; and that, by being too frequently electrified at a time, their virtue appeared to diminifh. I therefore judged it proper to be provided with changes of pairs; and that there might be the greater conformity between the experiments, I chofe them as exactly as poffible of the fame fize and fubftance. The fort I fixed upon, is what is called half gauze; the weight of the white ftocking, at an average, 18 *dwt.* and 10 *gr.* but when died black, 1 *oz.* and 1 *dwt.* the weight being increafed, by the dying of that colour, above 5 *dwt.* in the pair. When the white and the black ftocking were warmed at the fire, fo as to be prepared for electricity, they ufually loft about a twentieth part of their weight; fo that in the courfe of my experiments I rate the white at 17 *pwt.* and ½, and the black at 1 *oz.* The fcale, with the filk lines that belonged to it, and the hook, was adjufted precifely to the weight of 1 *oz*; and as I commonly meafured the ftrength of cohefion by fixing the hook to the black ftocking, and taking hold of the white, I had but to make an allowance of 2 *oz.* more than the weights put into the fcale, fo as to take the precife weight the ftockings could raife by the power of cohefion.

I meafured this power two different ways; the firft whilft the one ftocking was ftill within the other; fecondly, when feparated, and the one afterwards applied externally to the other. In the firft of thefe cafes, it may be thought that an allowance fhould be made for the friction in pulling the ftockings afunder; but that appeared to me to be very inconfiderable; for

when

when thofe of the fame colour were put one within the other, and inverted, they dropped afunder of themfelves; or if there was any intanglement about the heel, a little fhaking difengaged and feparated them: however, if it fhould be thought proper, the allowance of an ounce may be made, by deducting fo much from the weight refpectively found.

I have but one circumftance more to mention, before I proceed to give the refult of my experiments; which is, that we muft not be furprifed, when we find the force of cohefion externally, to be in no regular proportion with that within; for when the ftockings are highly electrified, they rufh together with fuch impetuofity, that it is extremely difficult to direct their motion, and make them meet in the manner moft advantageous for cohefion.

In the experiments I have made, to meafure the force of electrical cohefion, I have always found it anfwerable, fo far as I could judge from appearances, to the degree of electricity at the time excited. When the ftockings have been but weakly electrified, I have found them unable to fupport the weight, the one of the other. When in a more powerful ftate of electricity, I have known them to raife, refpectively, from one to twelve ounces, and upwards; nay, once I found the cohefion fo ftrong as to move feventeen ounces, including the fcale and the black ftocking. For the fake of accuracy, I fhall give a particular account of the refult of a few of my moft remarkable experiments, as I find them in the notes, which I took at the time of making them.

The firft I find is of the 30th of March; the wind at north-eaft; the weather clear, inclining to

froft. The white ftocking within the black, raifed the weight of 1 *lb.* 1 *oz.* 6 *dwt.* ½, half a pennyweight more feparated them. I find no note here taken of the weight born by the ftockings applied externally to each other.

April 6. A hazy and wet morning; but the wind at north-eaft. With new ftockings; the white, being within the black, raifed 11 *oz.*—without, 6 *oz.* With another pair, that had been much ufed in experiments; the white, within, raifed 6 *oz.*—without, 3 *oz.*

April 16. A clear dry morning; the wind at north-eaft; Fahremheit's thermometer at 48. Of the new, the white ftocking, within, raifed 1 *lb.* 5 *oz.*—without, 6 *oz.* 15 *dwt.*—Of the old, the white ftocking, within, raifed 8 *oz.*—without, 5 *oz.* 15 *dwt.*——

The laft obfervation I find marked, is of the 19th of May; the weather clear; the wind at north-eaft; the thermometer at 55. The white, within the black, raifed 10 *oz.*—and without, 6 *oz.*

It is to be remarked, that by this time I had got the old ftockings wafhed; and now there appeared little or no difference between the power of them and of the newer pairs; though the latter had ftill the advantage.

In making the experiments, it was neceffary to lofe as little time as poffible, on account of the wafte of electricity: I could therefore feldom proceed to the accuracy of fractions of a penny weight; nor often indeed of thofe of an ounce; and this is the reafon that my notes run generally in round numbers, which, however, may be depended upon.

The

The greateſt weight I have been able to raiſe by the force of electrical coheſion, as appears above, has been ſeventeen ounces. Now the white ſtocking, which weighed but 17 *dwt.* and an half, bore all this weight: in this caſe therefore it raiſed, by the ſtrength of its coheſion with the black, 340 pennyweight; that is, nearly twenty times its own weight＊. And if we conſider that the force, applied to ſeparate them, acted in a direction parallel to the ſurfaces, by which they cohered; and that when the ſurfaces are ſmooth, a force acting in ſuch direction, has much greater influence in ſeparating bodies, by making them ſlide gently over one another, than if thoſe bodies were rigid, and the force employed to ſeparate them acted in a direction perpendicular to the cohering ſurfaces;

＊ As the experiments mentioned above were made in the ſpace of ſix or ſeven weeks from about the latter end of March, when the temperature of the air begins to be leſs favourable for electricity, I did not doubt, but upon the approach, and during the progreſs of the enſuing winter, I ſhould meet with inſtances of a ſtronger degree of coheſion, than I had before been able to aſcertain. Accordingly, ſince this paper was read in the Royal Society, and particularly in the months of November, December, and January laſt, at times, when the weather was clear and froſty, I found that the ſame ſtockings lifted conſiderably more, than I had been able to make them do in the preceding months of March, April, and May. I likewiſe found, that when the ſtockings were perfectly new, or the black dipt afreſh, and the white newly cleaned and ſulphured, as alſo, that when they were of a more ſubſtantial make, ſuch as thoſe that are wove of ſpun ſilk, weighing commonly about the double of thoſe that go by the name of half gauze, their power of coheſion, when favoured by the temperature of the air, increaſes to a very conſiderable degree. Under thoſe circumſtances, at particular times, I have been able to make the black ſtocking, or the white, when the rough ſides of each were put together, raiſe (the half gauze) from 20 to 40, and (of ſpun ſilk) from 40 to 90 times its own weight. Vide the ſubſequent letter from Dr. John Mitchell to the Rev. Dr. Birch.

when

when we confider this, I fay, it will be hard to de-
termine how great the ftrength of their cohefion may
be.

The experiment may be tried with two plates of
glafs, the one electrified *pofitively*, the other *nega-
tively*. As in that cafe the principle, upon which
the cohefion of the white with the black filk depends,
would take place, I make no manner of doubt but
the effect would be the fame. I have not had an op-
portunity of trying the experiment: but I fhould ex-
pect that the two plates would be found to cohere
with greater force, than I have been able to afcertain
in the ftockings ; as a contrivance might be made to
pull them afunder in a direction perpendicular to
their cohering furfaces.

The force with which the black and the white
ftocking cohere, is not the only thing remarkable in
their junction. The folution of that cohefion, and
the different degrees of tenacity, according to differ-
ent circumftances, afford fome curious obfervations.

When the black and the white ftocking are in co-
hefion with each other, if another pair, more highly
electrified, be feparated, and prefented to the former
ftill in conjunction, the black to the white, and the
white to the black ; in that cafe, the cohefion of the
firft pair will be diffolved, and each ftocking of the
fecond, will carry off that of its oppofite colour ad-
hering to it. If the degree of electricity of both
pairs be equal, the cohefion of the firft pair will be
weakened, but not diffolved ; and all the four will
cohere, forming as it were one mafs. If the fecond
pair be but weakly electrified, the cohefion of the
firft pair with one another will be but little impair-
ed,

ed, and that of the ſtockings of the ſecond with thoſe of the firſt, will be weak in proportion. And laſtly, if the ſecond pair be not at all electrified, or if, in their place, any other body not electrified be preſented, there will be no effect produced on either hand.

White ſilk and black, when electrified, not only cohere with each other in the manner ſhewn above, but when in a high degree of electricity, are found, both one and the other, to adhere to bodies of broad and even, or poliſhed ſurfaces, though thoſe bodies be not electrified. This adheſion I diſcovered accidentally. While I was about ſome electrical experiments, having, without deſign, thrown a ſtocking, that was highly electrified, haſtily out of my hand, I was ſurpriſed to find it ſome time after, ſticking againſt the paper-hangings of my room. This led me to make the following experiments.

I preſented the white and the black ſilk, highly electrified, and in coheſion with each other, to the hangings; but no effect was produced. I then ſeparated the black from the white, and preſented them ſingly; in that caſe each of them readily adhered to the hangings, which they likewiſe did when flung from a little diſtance, and continued there for near an hour before they dropped. Having ſtuck up the black and the white, in the manner above-mentioned, I came with another pair of ſtockings highly electrified, and applying the white to the black, and the black to the white, I carried them off from the wall hanging on thoſe that had been applied to them. When the ſecond pair were electrified, but to a moderate degree, on applying them, in the manner above

defcribed, the former immediately quitted their hold of the hangings, and dropped to the ground. The fame experiments held with the painted boards of the room ; and likewife with the looking-glafs ; to the laft of which, both the black and the white filk, appeared to adhere more tenacioufly, than to either of the former.

I am afraid I have prefumed too much on the patience of the Society, by giving fo full a detail of my experiments and obfervations, on a branch of electricity, that takes its rife from fo fingular, and fo mean a fubject as that of black and white filk : But however particular the fubject may be, the properties of electricity that are thence experimentally deduced are of a general nature, and muft find a place in every true fyftem of electricity. If any apology be wanted, the beft I can make, is to endeavour to draw fuch inferences from the experiments above recited, as may poffibly throw new light upon the theory of electricity.

But left I fhould fwell this paper to too large a fize; and as the feafon is at hand when the Society ufually adjourns for fome months; I think it beft to defer any matter I may have to offer, concerning the theory of electricity, till a convenient time after our next meeting. I fhall only beg leave at prefent, to mention one or two things, which I have not hitherto had a proper opportunity of throwing out.

It hath been faid, that the influence of colours in electricity is no new difcovery ; that Monf. Du Fay had treated at large upon the fubject; and, after a variety of experiments, had concluded, that colours, as fuch, had no effect in electricity ; but that whatever

was

was remarkable in that way, was owing to the change, which the ingredients of the dye produced upon the coloured body. I had not then feen what Monf. Du Fay wrote upon the fubject; but as I have fince, I fhall take the liberty to ftate this matter fairly.

The late Monf. Du Fay, an ingenious member of the Academy of Sciences at Paris, to whom we owe fome valuable difcoveries in electricity, gives an account of what is here alluded to, in a memoir, prefented in the year 1733. Electricity was at that time in its infancy; Mr. Hawkfbee had, but a little before, publifhed an account of his experiments; which brought fuch furprifing appearances of electricity to light, as could not but induce the curious to turn their eyes upon that fubject. In the courfe of thofe experiments, he had taken notice of fomething remarkable with regard to colours. Mr. Gray fucceeded, and having opened a new path, made ftill further difcoveries in electricity: he likewife, in giving an account of what he had obferved, hinted at fomething curious with regard to colours. But neither of them appear to have come to any determined point in this matter. Monf. Du Fay, who concurred with Mr. Gray, in carrying on electrical difcoveries, with a candour and ingenuity that did honour to them both, having entered upon an enquiry (the fubject of the memoir above-mentioned) to determine what fort of bodies were moft fufceptible of electricity, thought proper, in confequence of what had fallen from Mr. Hawkfbee and Mr. Gray, to examine what effect the different colours had in augmenting or diminifhing the electricity of different fubftances.

Accordingly

Accordingly he ranged a number of ribbands, of all the primitive colours, hanging them in the same vertical plane; and to these he applied an excited glass tube, in an horizontal direction.—Upon this he observed, that the black was first attracted; and, as he brought the tube nearer, the white next; and the rest successively, though not always in the same order. He made another experiment, in the same view, with gauzes of different colours, through which he tried the force of an excited tube, upon light bodies placed at a proper distance behind them : and from the result he was of opinion, there was something in the influence of colours. But having afterwards tried some experiments with the coloured rays of the sun as refracted by a prism, with flowers of different colours, and with white ribbands rubbed over with differently coloured substances, he began to change his opinion. He likewise had recourse to what he calls a decisive experiment: he dipped his different-coloured ribbands in water; and when they were all equally wetted, he applied his tube, and found they were all equally 'attracted. From this last-mentioned experiment, in particular, he concluded that colours, as colours, had no effect in electricity; but that all was owing to the ingredients of the dye imbibed by the coloured body.

It is not my purpose here to inquire, whether Monf. Du Fay's conclusion is well or ill founded. Whatever may be the decision of that point, I apprehend the whole of this affair hath very little concern with what hath been the subject of these papers, and could have been of little use to me, had I been acquainted with it before.

The

The feries of my experiments, however inconfiderable they may prove in the refult, have taken a turn I did not at firft forefee. I fet out with inquiring into the nature of an electricity, that feemed to have fome connexion with the human body: I had made but little progrefs when I was furprifed with an appearance of electricity, arifing from the contraft of two colours, or coloured bodies of the fame fubftance: I met with it in my experiments, perpetually *pofitive* and *negative*; and under that appearance have followed it through a variety of its operations. The notions it hath led me to conceive, are different from thofe I had before entertained of electricity. Such as they are, they fhall, in due time, be freely fubmitted to the judgment of the Society.

Having been told by one or two of my friends, that they had tried fome of the experiments mentioned in the preceding papers, but could not get them to fucceed; I beg leave to add a few words, before I conclude, by way of caution to any, who may have a curiofity to verify my experiments.

However eafy it may feem to be, to follow the directions I have already given in electrifying the ftocking, I am fenfible from my own experience, that an attention to a number of little circumftances, befides fome fmall degree of addrefs, is requifite, in order to make it fucceed readily. This is known to have been the cafe with the electrical tube and globe: few people, at firft, knew how to manage them fuccefsfully; and yet glafs is not fo much expofed to many inconveniencies, that affect electricity, as filk. To give but one inftance of this: we know that a very fmall quantity of duft, greafe, or any other matter that

sticks to the tube, will injure its electricity: Now this is what may easily be wiped off from glass; but it may be very difficult to clear the silk of it, as silk is of a nature more apt to imbibe, and to retain extraneous matter than glass.——From these considerations, I should not be surprised at any persons being disappointed, as to the success of their experiments, in a few trials; but I should think it not fair for them to conclude from thence, that those above-recited are not to be depended upon *. Yet, if any member of the Society, who is curious in these matters, hath not been able to satisfy himself with regard to any particular experiment, I shall think it no trouble to shew him, at any proper time, how it succeeds with me.

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* The circumstances necessary to be attended to, in order to procure the most considerable appearance of electricity, are as follow. 1º. That the wind be in the north, north-east, or east; the weather clear and frosty, or inclining to frost. 2º. That the stockings be of silk; one of a good black, the other of a clear white; and both of the same size and substance. 3º. That they be new; or be kept carefully wrapt up in paper, and only used in electrical experiments. 4º. And lastly, That they be dried before the fire, and warmed to about the degree of heat of the human body, before they be drawn upon the hand or leg. And when taken off, care should be had in separating them, so as to keep them at a distance from one's clothes, or any thing that may lessen their virtue. The circumstances above-mentioned, might be gathered from what has been said in these papers; but I thought it not amiss to bring them together in this note.

*New Experiments and Obſervations concerning
Electricity.*

PAPER IV.   PART I.

Of Two diſtinct Powers in Electricity.

I Obſerved, in the papers I have already
had the honour of communicating to
the Society, that the different ſtate of electricity, as
diſtinguiſhed into *poſitive* and *negative*, was a leading
circumſtance in all electrical appearances; and that,
if followed out with due attention, it might bring us
to a clearer view, than we yet have had, of the
powers that nature employs in thoſe wonderful ope-
rations. Nothing could be more proper to ſerve this
purpoſe, than the ſimple apparatus I had all along
made uſe of. It conſiſted of two electric bodies, of
which, when they were excited, one never failed to
aſſume the poſitive, the other the negative ſtate of
electricity. The obſervations I had frequent oppor-
tunities of making, with regard to the manner in
which they acted, in theſe different ſtates, on other
bodies, and on each other reciprocally, confirmed
me in a notion, which, in the courſe of my expri-
ments, I had very early conceived.

My notion is, that the operations of electricity do
not depend upon one ſingle poſitive power, according
to the opinion generally received; but upon two
diſtinct, poſitive, and active powers, which, by con-
traſting, and, as it were, counteracting each other,

produce the various phenomena of electricity; and that, when a body is said to be positively electrified, it is not simply that it is possessed of a larger share of electric matter than in a natural state; nor, when it is said to be negatively electrified, of a less; but that, in the former case, it is possessed of a larger portion of one of those active powers, and in the latter, of a larger portion of the other; while a body, in its natural state, remains unelectrified, from an equal ballance of those two powers within it.

I do not here undertake to give a system of electricity. It is the design of this paper to propose the grounds of my opinion, only so far as it rests upon observation and experiment. If the proofs I have to offer shall be found conclusive, and it be allowed, that two distinct and counteracting powers prevail in electricity, one of them corresponding with the positive, and the other with the negative state; in that case, the truth thus established, may afterwards be assumed as a principle in theory; and we may try how far it will serve in accounting for the various appearances of electricity. But even then, I should not think 'of troubling the Society with discussions of so great a length, and of so speculative a nature, as those usually are that relate to theory.

I might bring arguments to prove the existence of two distinct powers in electricity, from a variety of observations and experiments, some of them taken from among those mentioned in the preceding papers: But as the bounds I prescribe to myself in this, do not permit me to enter upon so large a field, I shall confine myself to such observations only, as have fallen

within

within my notice, in one particular branch of electricity, that which relates to the Leyden phial.

All who allow of *positive* and *negative* electricity, know, that the phial, when charged, exhibits electricity in those two states, the one within, the other on the outside; and that when a communication is made between the two, by the means of a non-electric touching the coating, and at the same time approaching the wire, or *vice verfâ*, the explosion is produced, and the phial discharged. This reduces the question to a narrow compass; for if, upon the discharge of the phial, we meet with proofs not only of a power acting from within to the outside, but also of a power acting at the same instant from the outside to within, then, I think, we may fairly conclude, that what is called *negative* electricity is, in reality, a positive active power; and that electricity, in general, consists not of one alone, but of two distinct, positive powers, acting in contrary directions, and towards each other.

The proof I shall offer first, is founded on the following experiment. When the phial is electrified but a little, if we touch the coating of it with a finger of one hand, and at the same time approach a finger of the other hand to the wire, we shall receive a pretty smart blow upon the tip of each of the fingers, the sensation of which reaches no farther: If the phial be electrified a degree higher, we shall feel a stronger blow, reaching to the wrists, but no farther: When again it is electrified to a still higher degree, a severer blow will be received; but will not be felt beyond the elbows: Lastly, when the phial is strongly charged, the stroke may be perceived in the wrists and elbows;

but

but the principal fhock is felt in the breaft, as if a blow from each fide met there. This plain and fimple experiment feems obvioufly to fuggeft to obfervation, the exiftence of two diftinct powers, acting in contrary directions : And I believe it would be held as a fufficient proof by any who fhould try the experiment, with a view to determine the queftion fimply from their own perceptions.

But as I am fenfible, that the proof of any important point in philofophy, ought not to depend upon the perceptions of this or that particular perfon, I judged it neceffary to have recourfe to experiments, the refult of which might admit of no ambiguity. The fortunate difcovery of M. Mufchenbroek and M. Allamand, with the improvements that have fince been made upon it, puts it in our power to increafe electricity to what degree we pleafe. I did not therefore defpair of the means of bringing this matter to a fair decifion. I expected, that if an electrical ftroke fhould be made to pafs through a folid body, with fo much force as to pierce and tear the fubftance of it, fuch marks would be left, as might enable us, with certainty, to trace the courfe of the electrical power in its paffage through the body.

Having no apparatus of my own capable of producing fuch effects, I had recourfe to a worthy member of this Society, doctor Franklin, who was poffeffed of a very good one. I had communicated all my obfervations to this gentleman as they occurred, and, in return, met with an ingenuity and candour, that render him as eftimable in private life, as the improvements he has introduced into electricity, and particularly his difcovery in relation to thunder

and

and lightning, will render his reputation lafting in the learned world. We differed in opinion with regard to the point in queftion; neverthelefs I found him ready to give me all the affiftance in his power, for bringing the matter to a fair decifion. I had feen him pierce a quire of paper with a ftroke of electricity; and as I perceived it had been ftruck feveral times before, I defired he would give it me, that I might at leifure examine the effects of the fundry ftrokes.

When I came to do fo, I obferved, that at every hole which had been made through the quire, the upper and the under leaf (for the quire had been laid in an horizontal pofition when it was ftruck) were ragged about the orifice, and thofe ragged edges pointed moftly outwards from the body of the quire. But what was more material; when I came to turn over the leaves, I found, that the edges of the holes were bent regularly two different ways (and more remarkably fo about the middle of the quire), one part of each hole upwards, and the other part downwards; fo that, tracing any particular hole as it traverfed the quire, I found on one fide the fibres pointed one way, and on the other fide the other way; much in fuch a manner, as if the hole had been made in the quire, by drawing two threads in contrary directions through it.

This was not all: A piece of paper, covered on one fide with Dutch gilding, had been accidentally left between two leaves in the quire, and had been pierced by two different ftrokes. This exhibited a very remarkable appearance: Where each of the ftrokes had been given, the gold leaf was ftripped off, and had
left

left the paper bare for a little fpace, in an oblong form, rounded at the ends; in which, at the diftance of about a quarter of an inch from each other, appeared two points, one of them a little round hole, the other only an indent or impreffion, fuch as might have been made by the point of a bodkin In the leaf, which fronted the gilding, two fuch points likewife appeared, correfponding to thofe above-mentioned; fo that the hole in the one was oppofite to the impreffion in the other, but furrounded with little black or blueifh circles. When the hole, which had been ftruck in the quire, was traced from above down to the gilding (for the gilt paper happened to lie with its gilded fide uppermoft), it was found to terminate on the point in the gilt paper where the impreffion appeared, and there the impreffion pointed downwards. Again, when the hole in the lower part of the quire was traced from below upwards, it was found to terminate on the point in the leaf fronting the gilding, where the impreffion was, and there the impreffion pointed upwards. The facts above-mentioned feem to leave it without doubt, that the ftroke had been given, at the fame inftant, upwards and downwards; but that the electrical power from above, and from below, had feized upon the gilding, diffipated part of it in vapour, and by that means become fo weak, that each of them could afterwards only make an impreffion upon the paper, marking the refpective directions of their courfe.

I communicated thefe obfervations to Dr. Franklin; but as no conclufion can, with certainty, be drawn but from facts, confirmed by repeated trials, I defired to have the fatisfaction of making a few experiments

<div align="right">riments</div>

O

riments with him in relation to this matter; to which he readily confented. For that purpofe I waited upon him one morning about the middle of June laft; and the better to afcertain what was effential in the facts, I varied the circumftances a little from thofe above.

In the middle of a paper-book of the thicknefs of a quire, I put a flip of tin-foil; and in another of the fame thicknefs I put two flips of the fame fort of foil, including the two middle leaves of the book between them. Upon ftriking the two different books, the effects were anfwerable to what I expected. In the firft, the leaves on each fide of the foil were pierced, while the foil itfelf remained unpierced; but, at the fame time, I could perceive an impreffion had been made on each of its furfaces, at a little diftance one from another; and fuch impreffions were ftill more vifible upon the paper, and might be traced as point-ing different ways. In the fecond, all the leaves of the book were pierced, excepting the two that were between the flips of foil; and in thefe two, inftead of holes, the two impreffions, in contrary directions, were very vifible.

I have lately got an electrical apparatus of my own, formed on the model of that of Dr. Franklin's, and have had opportunity fince, of making frequent repe-titions of the experiments above-mentioned. Not-withftanding fome little variation in appearances, arifing, as in other electrical experiments, from the particular ftate of the weather, the different degree of electricity, or other accidental circumftances, I have met with nothing but what confirms me in my opinion of two diftinct counteracting powers. All

the remarks I have been able to make in the repetition of experiments, that need to be added to what I had before obferved, may be reduced to the three following.

1°. When a quire of paper, without any thing between the leaves, is pierced with a ftroke of electricity, the two different powers keep in the fame tract, and make but one hole in their paffage through the paper: not but that the power from above, or that from below, fometimes darts into the paper at two or more fundry points, making fo many holes, which, however, generally unite before they go through the paper. What I mean is, that I never yet could obferve the two powers to make different holes in the paper; but that they always keep the fame common channel, rufhing along it with inconceivable impetuofity, and in contrary directions. They feem to pafs each other much about the middle of the quire; for there the edges are moft vifibly bent different ways: whereas in the leaves near the outfide of the quire, the holes very often carry more the appearance of the paffage of a power iffuing out, and exploding into the air, than of one darting into the paper.

2°. When any thin metallic fubftance, fuch as gilt-leaf, or tin-foil, is put between the leaves of the quire, and the whole ftruck; in that cafe, the counteracting powers deviate from the directeft tract, and leaving the path they would in common have taken through the paper only, make their way in different lines to the metallic body, and ftrike it in two different points, diftant from one another about a quarter of an inch, more or lefs (the diftance appearing to be leaft when the power is greateft;) and whether they

pierce,

pierce, or only make impreffions upon it, in either
cafe, they leave evident marks of motion from two
different parts, and in two contrary directions. It is
this deviation from a common courfe, and the fepa-
ration of the lines of direction confequent upon it,
that affords us the ftrongeft proof, of the exertion of
two diftinct and counteracting powers.

3°. When two flips of tin-foil are put into the
middle of the quire, including two or more leaves
between them, if the electricity be moderately ftrong,
the counteracting powers only ftrike againft the flips,
and leave their impreffions there. When it is ftronger,
we generally find one of the flips pierced ; but fel-
dom both : and from what I have obferved in fuch
cafes, it would feem as if the power, which iffued
from the outfide of the phial, acts more ftrongly than
that which proceeds from within; for the lower flip
is moft commonly pierced : But that may be owing
to the greater fpace, the power from within has to
move through, before it ftrikes the paper.

I take the liberty to lay before the Society a paper-
book, of the thicknefs of a quire, ftruck three times in
the manner above defcribed. The firft ftroke (A) is
given, when there is nothing between the leaves of
the book. The fecond (B) when a piece of paper,
covered on one fide with what is commonly called
Dutch gilding, is laid in the middle. The third
(C), when two flips of tin-foil are put into the book,
including the two middle leaves between them.

The members, who are curious in thefe matters,
may, at their leifure, examine the effects of the fun-
dry ftrokes ; and if any gentlemen, in particular, de-
fire the farther fatisfaction of feeing the ftrokes given,

I

I fhall be very ready, at any proper time, to comply with their defire.

New Experiments and Obfervations concerning Electricity; *by* Robert Symmer, *Efq*; *F. R. S.*

PAPER IV. PART II.

Of Two diftinct Powers in Electricity.

Read Dec. 20, 1759. THE notion of two diftinct electrical powers, acting in contrary directions, may appear to fome to be the fame with that of the *effluence* and *affluence* of electrical matter, which M. l'Abbé Nollet gives as the general caufe of the phenomena of electricity *. It may therefore be not improper to take a nearer view of thefe two opinions, to fee how far they agree, and in-what they differ.

This ingenious author, whofe merit in the learned world is very confiderable, particularly with regard to his labours in electricity, had obferved, that, when a body is electrified, a current of electric fluid iffues from it, and, in the form of diverging rays, fpreads through the air, and enters into other bodies; and that, at the fame time, a current of electric fluid,

* " Plus de trois ans fe font écoulés depuis que j'ai propofé " comme la caufe generale des phenoménes électriques, *l'effluence* " *et l'affluence fimultanées d'une matiere fluide, prefente par tout*, *et* " *capable de s'enflammer par le choc de fes propres rayons.*" Prefacⷮ to *Recherches fur l'electricité*, at the beginning.

iffuing

iſſuing from other bodies, paſſes through the air, and, in the form of converging rays, enters into the body electrified. From thence he concludes, that a continued, and (to uſe his own terms) ſimultaneous * effluence and affluence of a fluid matter, extremely ſubtile, conſtitutes † electricity. Upon this principle he endeavours to account for all the phenomena that attend the electrification of bodies.

What M. l'Abbé Nollet has obſerved with regard to two contrary currents in electricity, is by no means inconſiſtent with the principle of two diſtinct counteracting powers. On the contrary, the exiſtence of two ſuch currents is, according to my opinion, a neceſſary conſequence of the exertion of thoſe powers from one body upon another. It is a phenomenon of electricity only; not the principle upon which all electrical appearances depend.

But a more eſſential difference takes place between this gentleman's opinion and mine: he repreſents the two currents as conſiſting but of one and the ſame

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* " Ces deux courans qui ont des mouvemens oppoſes, ont lieu " tous deux enſemble, c'eſt ce que j'exprime par le mot ſimul- " tanés." *Lettres ſur l'electricité*, p. 30.

† " L'electricité, comme je l'ai deja dit et prouvé ailleurs, n'eſt " pas ſeulment l'émanation d'une matiere qui s'elance du corps " électriſé ; c'eſt auſſi un remplacement continuel qui ſe fait de " cette matiere, par une autre tout-a-fait ſemblable, qui ſe porte " de toutes parts au corps électriſé ; c'eſt pour ainſi dire, un com- " merce de la matiere qui j'ai nommée effluente, et de celle que " j'ai appellée affluente. Si celle-ci vient à manquer, ou que la " premiere n'ait plus la liberté de ſortir, cet état ou *ce double* " *mouvement*, que l'on nomme *electricité*, doit bien-tot ceſſer." " *Eſſai ſur l'electricité*, p. 202.

fluid ;

fluid ‡; admits but of one kind of electricity ‖; and maintains, that two bodies cannot be faid to be differently electrified, but as they are electrified in a higher or lower degree * On the other hand, it is my opinion, that there are two electrical fluids (or emanations of two diftinct electrical poweis) effentially different from each other; that electricity does not confift in the efflux and afflux of thofe fluids, but in the accumulation of the one or the other in the body electrified; or, in other words, it confifts in the poffeffion of a larger portion of the one or of the other power, than is requifite to maintain an even ballance within the body; and, laftly, that according as the one or the other power prevails, the body is electrified in one or in another manner.

In thofe refpects we differ in opinion. Who is in the right is another queftion. The whole feems to turn on a fingle point, namely, whether there be but one, or if there are two diftinct kinds of electricity. The bounds of this paper do not permit me to enter upon a full difcuffion of the point. I cannot, however, but obferve, that the whole feries of experiments mentioned in the preceding papers, tends to confirm the diftinction, formerly made, of electricity into two kinds; and to fhew, that there is an effential difference (whatever it be that conftitutes that difference) between what is commonly called pofitive electricity, and negative. A farther proof of that difference arifes from the fuccefs of an experiment, of

‡ *Effai fur l'electricité*, p. 160, 161.
‖ Ibid. p. 118, 119.
* *Lettres fur l'electricité*, p. 101. 105,

which

which I threw out a hint in my third paper, and which I have since taken an opportunity of making, touching the electrical cohesion of glafs. The experiment is as follows:

I took two panes of common window-glafs, about nine inches fquare, the thinneft, the moft even, and the fmootheft in their furfaces I could get. I covered one of the fides of each with tin-foil, leaving the fpace of near an inch from the edges uncovered. I warmed them a little at the fire; and, applying the two bare fides together, I laid them upon four wine-glaffes, which fupported them at the corners. I then brought down a chain from the prime conductor, nearly to touch the coating of the upper plate, and applying a wire, which I held in my hand, to the coating of the under plate, the machine was put in motion, and the electrification performed, as in the cafe of the common electrical pane. When the operation was completed, I removed the chain and the wire, and taking hold of two oppofite corners of the upper glafs (thofe correfponding to them in the other having been purpofely cut away, I lifted it, and found, that the under glafs came up with it. The cohefion appeared to me to be confiderably ftrong; but I had not any proper apparatus ready to meafure the ftrength of it. I laid them down again on the wine-glaffes, and procured an explofion, as in the cafe of the common electrical pane. I then took hold of the corners of the upper glafs, and lifted it; but found, that the cohefion was diffolved, the under glafs remaining behind.

I could indeed perceive, that, after the difcharge, there was ftill fome fmall degree of cohefion between the plates, which felt as if fome glutinous fubftance

had

had got feeble hold of them : but this was no more than what I found took place between them, when, without being electrified, they were forced clofe together. For that reafon, two plates of glafs, finely polifhed, and fo even as to come into clofe contact through the whole extent of their oppofed furfaces, would be very improper for this experiment; for, when the power of electricity had forced them into contact, the preffure of the air, and a cohefion proceeding from another principle, would keep them together.

But to purfue the purpofe of our experiment.—All who admit of the diftinction of electricity into two kinds, agree, that as in the Leyden phial, fo likewife in the electrical pane, the different fides are differently electrified: That fide, which more immediately receives its electricity from the glafs globe, is faid to be pofitively, and the other negatively, electrified. What may be faid of the electrical pane, is applicable to the glafs plates in this experiment; for, when they are put together in the manner mentioned above, they form an electrical pane between them; one of the plates correfponding with one of the fides, and the other with the other fide of the pane. When, therefore, the glafs plates are electrified in the manner before defcribed, the plate, which receives its electricity immediately from the chain, will, according to this diftinction, be pofitively electrified, and that which receives its electricity from the wire, negatively.

Upon thefe confiderations, we may expect, from the experiment in hand, the means of determining, whether the diftinction of electricity into two different kinds is merely nominal, or if there is an effential

difference

difference between them: For after the glafs plates have been electrified in one pofition, fo as to be incapable of receiving any more electricity, if they be inverted, and in that new pofition prefented to the chain and wire, and the globe again be put in motion, according as one or other of thofe opinions holds, correfpondent effects will follow. If the electricity, that comes by the chain, be of the fame nature with that which comes by the wire, no change will be produced upon the plates; for being before full of one and the fame kind of electricity, they can do no more than keep what they had, or exchange it for juft as much of the fame kind. But if, by the chain and the wire, two kinds of electricity, totally different in their nature, be conveyed into the refpective plates, in that cafe it is to be expected, that the electricity that each of them had acquired in their former pofition, will be gradually deftroyed, till no figns of electricity appear in either; after which, they will begin again to be electrified, having their electricity reverfed.

In order to fee what would really happen, I repeated the experiment in the following manner: I electrified the two plates till they were fully charged, and in ftrong cohefion, the fnapping from the chain and the wire having totally ceafed. I then turned them upon the glaffes that fupported them, applied the chain and the wire to the different fides, and began to electrify as before. The glafs globe was no fooner in motion, than the fnapping from the chain and the wire returned with violence; and the plates, which, in the former pofition, would receive no more electricity, appeared, in their new fituation, to receive

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it both from above and below, more greedily than ever. As this new electrification proceeded, I found, by feveral trials, that the cohefion became gradually weaker, till, at a certain period, it was totally dif-folved; from which, it began again to be reftored, and at laft, when the fnapping ceafed, I found it to be as ftrong as before.

I carried the experiment ftill farther: I took two complete electrical panes (that is, two glafs plates, covered each on both fides with tin-foil), and laying one upon the other, I applied the chain and the wire, and proceeded to electrify. The electrification took place throughout; and I could procure an explofion from either of them fingle, or from both together: but however highly they were electrified, I never could perceive the leaft appearance of cohefion between them. This was agreeable to what I expected. I judged, that, in confequence of two different kinds of electricity, each of the panes would be charged, on its different fides, with the different kinds; which, by counteracting one another in the fame pane, would reduce it to a neutral ftate of electricity, and by that means prevent the two panes from acting on each other.

It is not here my purpofe to account for electrical cohefion; yet I cannot but obferve, that, in this cafe at leaft, it is obvious, that the cohefion cannot be owing to an *effluence* and *affluence* of one and the fame electrical fluid: For the two plates being of the fame fub-ftance, and in every refpect alike, the effluent current muft have juft as great an effect in feparating them, as the affluent can have in bringing or keep-ing them together. The experiment above feems to
make

make it evident, that there are two diftinct kinds of electricity; and the influence of thofe in making the plates cohere, feems naturally to denote, in concurrence with the experiments mentioned in the former part of this paper, two diftinct and counteracting powers, correfponding with the two different kinds of electricity.

If, upon the whole, the arguments I have brought to prove the exiftence of two diftinct powers in electricity, are found to be conclufive, it may, perhaps, be expected of me to fay fomething of the nature of thofe powers. Without entering into any particular theory, or indulging myfelf in loofe conjecture, I fhall take the liberty to offer a few confiderations, fuch as occur to me on this occafion.

All we know of active powers, extends no farther than as we perceive them to be effects of a power ftill more general; or as we find them producing effects according to certain laws. I have not been able to trace the powers of electricity farther back, than the obfervations I have given above, have led me. I do not therefore take upon me to determine, whether they confift of the finer parts of matter, conftituting an active and elaftic fluid, the elafticity of that fluid remaining ftill to be accounted for; or if they are of a fubftance yet more fubtile and active, of which, however, we have hitherto been able to form no diftinct idea. Whatever other power they may be the immediate effects of, or whatever be the fecret and imperceptible manner in which they act, the more interefting object of our inquiry, is to know the laws according to which they act, and how far their operations extend in the material world.

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The fame obfervations that lead us to the difcovery of any power, if followed out with due attention, may ferve to inftruct us in the laws of its action; and if we can attain a fufficient knowlege of thofe laws, however ignorant we may be of the peculiar manner in which the power exerts itfelf, we may be, by that means, enabled to trace it through its various operations.

The laws of nature are few and fimple. It is only from the variety of circumftances, under which the refpective powers are exerted, that the phenomena of nature are multiplied. The powers of mechanic motion, thofe I mean by which bodies act upon one another in impulfe or preffure, are found to be under the direction but of three general laws; and from thence is deduced a moft extenfive branch of natural knowlege.

The laws that regulate the powers, by which bodies act at a diftance upon one another, when we come to be better acquainted with them, may be found to be not more numerous or complex. What appears wonderful to us, is, that bodies fhould at all be capable of acting upon one another at a diftance: But are we not equally ignorant of the manner, in which the powers of motion are exerted, when the bodies are in actual congrefs? Daily experience convinces us of the fact in this cafe; and in the other, repeated experiments, and frequent obfervations, leave us little room to doubt, that there are powers, which, when lodged in one body, are capable of being exerted upon another at a diftance.

The powers of electricity are found to be of this nature. When either of thofe powers prevails in a

7   body,

body, it exerts its influence, fo long as it is lodged there, every way around, and, by preffure or otherwife, acts upon another body within the fphere of its influence. If it meets with no refiflance, it enters this other body, and from this begins to act, as it did from the former. In the mean while, the counteracting power prevailing in this other body, the like effects will be produced: and thus thefe two bodies, by means of their correfponding powers, will continue to act upon each other, till their powers be reduced to an equal ballance.

Did electricity confift but of one power, after an experimental inquiry into the laws according to which it acted, we might, upon that principle, be able to account for the phenomena of electricity. But if two diftinct and counteracting powers prevail, as appears to me to be evident from the preceding experiments and obfervations, in that cafe, it will be impoffible to give a complete and confiftent theory of electricity, but upon the principle of two fuch powers. Nor will this principle be found, upon due confideration, to difagree with the general fyftem of nature. It is one of the fundamental laws of nature, that action and re-action are infeparable and equal. And, when we look around, we find that every power, that is exerted in the material world, meets with a counteracting power, that controls and regulates its effects, fo as to anfwer the wife purpofes of Providence.

A Letter to the Reverend Dr. Birch, *Secretary to the Royal Society, concerning the Force of electrical Cohefion.*

S I R,

I Happened to be at Mr. Symmer's on Saturday the 15th inftant, when he defired me to be witnefs to fome electrical experiments, he was about to make, with filk ftockings, of a particular kind, which he had received for that purpofe.

The weather was then remarkably favourable for electricity, being clear and dry, with a fharp froft, which had continued five or fix days. The wind was eafterly, and had been in that quarter for ten days. It was about noon when we made our experiments; the barometer at 30, and Fahrenheit's thermometer at 32.

The ftockings above-mentioned were wove of carded and fpun filk, and were more fubftantial and weighty, than thofe with which he had made the experiments mentioned in his third paper. One pair was of a deep black, having been twice dyed, in order to improve the colour. Another pair was of the natural colour of the filk, of a dufky white; and both new. The pair of black weighed four ounces, eight pennyweight, and four grains; and the white three ounces, eighteen pennyweight, and fifteen grains.

We began with making a few experiments with the thin ftockings formerly made ufe of; and found the refult to be much the fame with what is related

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by Mr. Symmer in his third paper : that is, we found,
that when the white ſtocking was put within the
black, or *vice verſâ*, and both highly electrified,
taking hold of the one, while a ſcale with weights
was put to the other, we could raiſe ſeventeen ounces
before the ſtockings ſeparated.

We then repeated one or two of thoſe experiments
with ſome little variation of circumſtances. We
turned one of the ſtockings inſide out, and put that
within the other : the inner or rough ſides of the
ſtockings being thus together, by which means they
took faſter hold of each other, we now found, that
it required the weight of twenty ounces to ſeparate
them.

When the ſtockings were ſeparated, and applied
externally to each other, they then raiſed the weight
of ten ounces *.

We

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* Some time after this letter of Dr. Mitchell's had been read in
the Society, as I was of opinion, that the thin ſtockings, mentioned
above, had loſt much of their electrical power ſince the beginning
of April laſt, when they firſt had been made uſe of in experiments,
and that it was owing to the peculiar influence of the weather, that
they raiſed as great a weight now as they had done then, I had
the black new dyed, and the white waſhed, and afterwards whit-
ened in the fumes of ſulphur. Upon this, I found their force very
much increaſed. On the 9th of January, the weather being much
in the ſame ſtate as it had been the 15th of the preceding month,
the ſtockings, thus prepared, and put one within another, having
their rough ſides together, lifted no leſs than three pounds and three
ounces before they ſeparated. Dr. Mitchell was likewiſe preſent at
this experiment.

How far the circumſtance of ſmoking the white ſtocking in the
fumes of ſulphur, might have contributed to increaſe the electrical
power, is what I cannot take upon me to ſay. I ſhould think,
however, that it does not much contribute to it ; for we find, by
the experiments in the ſequel of this letter, that the force of cohe-
ſion

We next proceeded to try the force of electrical cohesion, with the stockings of a more substantial make; viz. those I have above described; and there we found it to be much more considerable, as appears by the following experiments.

1° When the white stocking was put within the black (without either of them being turned inside out), so that the outside of the white was contiguous to the inside of the black, they lifted nine pounds, wanting a few pennyweight. Now, taking the weight of the stocking to be one ounce, eighteen pennyweight, and fifteen grains (viz. the half of the weight of the pair as mentioned above), it follows, that, by the force of its cohesion with the black, it raised fifty-five times its own weight.

2°. When the white was turned inside out, and put within the black, their inner or rough sides being contiguous, they lifted no less than fifteen pounds, one pennyweight and a half, before they separated : So that, in this case, the single stocking raised ninety-two times its own weight †.

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sion is surprisingly great between the black and the white stocking of spun silk, when electrified : and yet I was assured by the hosier, that the white had never been put into the fumes of sulphur ; and that the colour it had was the natural colour of the silk, no other method having been taken to whiten it, than that of scouring and washing.

† Since that time, I have not been able to raise above ten or eleven pounds with these stockings, even when the weather has been most favourable ; owing, perhaps, to my having cut off all the ends of threads, and tufts of silk, which had been left on the inside of the stockings ; which I did with a view of increasing the cohesion : whereas, when the inner sides of the stockings were put together, those ends of threads, and tufts of silk, by joining intimately with those of the different colour, probably contributed much to produce that powerful cohesion.

3°.

3°. When the inner ftocking was drawn out, and applied to the outfide of the other, they lifted one pound and three quarters; that is, between ten and eleven times the weight of the white ftocking *.

It is not my defign to draw any conclufions from thefe experiments, and much lefs to determine how far electrical cohefion may ferve, as a principle, to to account for many remarkable appearances in nature. I relate the experiments I have been witnefs to, by way of fupplement to Mr. Symmer's third paper; and I confider the refult of them, only, as a farther proof of the furprifing degree, to which a power in electricity, which had not before been attended to, may be carried, in even the flighteft fubftances, thofe of white and of black filk. I am, Sir,

Your moft obedient, humble fervant,

Kew, December 18, 1759. John Mitchell.

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* In the third of thefe papers I obferved, that ftockings electrified, and applied to one another externally, cohered with a force greater or lefs, according to the manner in which they joined in contact with each other. This appears to be the reafon, why the ftockings here made ufe of, being much lefs pliable than the thinner kind, do not, in external cohefion, raife a weight fo great in proportion as thofe do. From thence I fancied, that if the ftockings of fpun filk fhould be firft allowed to come together, and afterwards be preffed clofe between one's hands, their cohefion externally with one another would thereby be much improved : Accordingly, upon repeated trials, I found, that the white ftocking, when thus preffed to the black in external contact, was capable of raifing between three and four pounds; that is, about twenty-two times its own weight.

I have taken the liberty to fubjoin thefe few obfervations, by way of notes, to Dr. Mitchell's letter, as they relate to the fame fubject, and contain matter, which has occuried fince his letter was read in the Society.

February 1ft, 1760. R. Symmer.

XXXVII. *Some Observations relating to the* Lyncurium *of the Ancients*; *by* William Watson, *M. D. F. R. S.*

TO determine the substance, denominated *Lyncurium* by the ancients, has been the occasion of much controversy among the more modern naturalists; some of whom, as late Dr. *Woodward*, believed it to be a species of the *belemnites*; others, as the late * M. *Geoffroy*, considered it as *amber*. But it is evident from † *Theophrastus*'s description of the *Lyncurium*, which is the most complete that has been handed down to us, that neither the one nor the other of the before-mentioned substances could be what he intended. His words are, Καὶ τὸ λυγκύριον. ᾗ γαρ εκ τέτε γλύφεῖαι τα σφραγίδια. ᾗ ἔςι ςερεολάτη, κάθαπερ λιθ⊙. ἕλκει γαρ ὥσπερ τὸ ἤλεκῖρον. οἱ δε φάσιν 8 μόνον κάρφη ᾗ ξυλον, αλλὰ ᾗ χαλκὸν ᾗ σίδηρον, εαν ἤ λεπῖος. ὥσπερ ᾗ Διοκλῆς ἔλεγεν. Ἔτι δὲ διαφανή τε σφόδρα ᾗ πυῤῥα. .... γίνεῖαι δὲ ᾗ καῖεργασία τίς αυῖε πλειων. From hence we learn, that " the *Lyncurium* was a stone
" used for engraving seals on: that it was very hard:
" that it was endowed with an attracting power like
" amber: and that it was said, and by *Diocles*
" among the rest, to attract not only straws and
" small pieces of wood, but also copper and iron, if
" beaten very thin: that it was pellucid, and of a

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* Mater. Med. Vol. I. p. 165. de succino.
Idem et Lyncurium quoque dicitur.
† Theophrastus περὶ τῶν λίθων.

" deep-

" deep-red colour ; and required no fmall labour to
" polifh it." The reft of *Theophraftus*'s defcription
is taken up with the fabulous account of the genera-
tion of this ftone, " that it is formed by the urine of
" the lynx, which the animal, as foon as it parts
" with it, hides, and fcrapes the earth together over
" it; and that the ftones vary according to the fex
" and difpofition of the animal."

Diofcorides *, in his hiftory of the *Lyncurium*,
gives us only the fabulous hiftory of its generation,
before mentioned by *Theophraftus*; and fubjoins, that
it is called by fome Ἤλεκτρον πτερυγοφόρον; that is,
amber, which attracts feathers to it.

Pliny, in his hiftory †, difbelieves both the fabu-
lous account of the generation of the *Lyncurium*, as
well as its attractive quality, related both by *Diocles*
and *Theophraftus*, and confiders the whole as a falfity;
though he is candid enough to confefs, that neither
himfelf, nor any one elfe in that age, had feen a gem
of that appellation.

Theophraftus, though more ancient, is, in moft
particulars, more to be depended upon than either
Diofcorides or *Pliny*. He ought to be confidered
much more of an original author, and one who wrote
from his own knowlege, than the others, who, va-
luable as they are, muft be regarded, in moft refpects,
as compilers. His account, then, of the appearance
and properties of the *Lyncurium* muft be confidered,
in order to examine, if any fubftance, known in our

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* Lib. II. cap. c.
† Plin. Hift. lib. XXXVII. cap. iii. Ego falfum id totum arbi-
tor, nec vifum in ævo noftro gemmam ullam eâ appellatione.

time, anfwers his defcription. But, firft, it is plain that Dr. *Woodward*'s hypothefis of the *belemnites* being the Lyncurium, was ill founded; inafmuch as the belemnites is neither pellucid, nor fit for engraving feals upon, on account of the friability of its texture; neither can it, by any management, be made to attract ftraws, chips of woods, or other light bodies. Nor is *Geoffroy*'s opinion lefs liable to exception; as amber, though it has the attractive power mentioned by Theophraftus, yet it has by no means the firm texture requifite to have feals engraved upon it; neither is it fo very hard, as is exprefly faid by this author concerning the Lyncurium, as to require great labour in polifhing it. Add to thefe, that *Theophraftus* has given a *particular account* of the hiftory and properties of *amber* * feparately, in the before-mentioned work.

If, after what has been faid, I may be permitted to give my thoughts concerning the *Lyncurium* of the ancients, I make no fcruple to think it to be exceedingly probable, that what we now call the *Tourmaline was the Lyncurium* of Theophraftus, as it agrees with that author's defcription in all its fenfible qualities; to wit, that it is a very hard pellucid ftone, of a deep-red colour; that it is very proper to engrave feals upon; that it attracts, like amber, not only ftraws and light pieces of wood, but filings of iron and brafs, as has been lately evinced by many experiments. And what will give fome weight to this

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* Vide Theophraft. περὶ τῶν λίθων. νγ.
Καὶ τὸ ἤλεκτρον λίθος. κỳ γαρ ὀρυκ]ον τὸ περὶ Λιγυστικήν. κỳ τ8 ωνὰν ἤ τ8 ἕλκειν δ'ὑναμις ἀκολ8θειν.

opi-

opinion is, that this ftone, though not much attended to by us till very lately, is very common in feveral parts of the Eaft Indies, and more particularly in the ifland of Ceylon, where it is called by the natives *Tournamal.*

The firft account which we have had, of late years at leaft, of this extraordinary ftone, was in the Hiftory of the Royal Academy of Sciences of Paris, for the year 1717; where we are told, that Mr. *Lemery* exhibited a ftone, which, he faid, was not common, and came from Ceylon. This ftone attracted and repelled little light bodies, fuch as afhes, filings of iron, bits of paper, and fuch like. The publifher of that hiftory then proceeds to give fome reafons for thefe phænomena. *Linnæus,* in his preface to the *Flora Zeylanica,* mentions this ftone under the name of *lapis electricus;* and takes notice of M. Lemery's experiments before-mentioned.

Notwithftanding this, no further mention was made of this ftone, and its effects, till very lately. The duke *de Noya,* in his letter to M. de Buffon, which was prefented to the Royal Society a few months ago, informs us, that when at Naples in the year 1743, the late count *Pichetti,* fecretary to the king, affured him, that, during his ftay at Conftantinople, he had feen a fmall ftone, called a tourmaline, which attracted and repelled afhes. This account the duke *de Noya* had quite forgot; but, being laft year in Holland, he faw and purchafed two of thefe ftones, which are there called *afchentrikker.* The making experiments with thefe called to his remembrance what formerly had been told him by count *Pichetti.* With thefe ftones he made, in company with Mef-
fieurs

fieurs *Daubenton* and *Adanfon*, a great number of experiments, of which the duke has favoured the public with a particular account.

In the year 1757, there were two accounts publifhed upon this fubject: the one is a memoir of M. Æpinus, read to the Royal Academy at Berlin, intituled, *De quibufdam experimentis electricis notabilioribus*. The other is a treatife in quarto, printed at Roflock, intituled, *Difputatio de electricitatibus contrariis. Auctore Joanne Carolo Wilke*. Since which time, Dr. Heberden, who is ever defirous of extending the bounds of fcience, having procured fome of thefe ftones from Holland, a great number and variety of experiments with them have been made here, particularly by the ingenious Mr. Wilfon; an account of which he has very lately communicated to the Royal Society.

XXXVIII. *An Attempt to account for the regular diurnal Variation of the horizontal magnetic Needle; and alfo for its irregular Variation at the Time of an Aurora Borealis: By* John Canton, *M. A. and F. R. S.*

Read Dec. 13, 1759. **T**HE late celebrated Mr. George Graham made a great number of obfervations on the diurnal variation of the magnetic needle, in the years 1722 and 1723; but declared himfelf ignorant of the caufe of that variation, in N° 383 of the Philofophical Tranfactions, where many of thofe obfervations are to be found. About

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the year 1750, Mr. Wargentin, fecretary of the Royal
Academy of Sciences in Sweden, took notice, both
of the regular diurnal variation of the needle, and alfo
of its being difturbed at the time of an aurora borea-
lis, as recorded in the 47th volume of the Philofo-
phical Tranfactions; but is filent as to the caufe. I
had no opportunity of making obfervations of this fort
myfelf, till the latter end of the year 1756; but,
fince that time, I have made near four thoufand,
with an excellent variation-compafs, of about nine
inches in diameter. The number of days, on which
thefe obfervations were taken, is 603; and the diur-
nal variation on 574 of them was *regular*; that is,
the abfolute variation of the needle weftward, was *in-
creafing* from about eight or nine o'clock in the morn-
ing, till about one or two in the afternoon, when the
needle became ftationary for fome time; after that,
the abfolute variation weftward was *decreafing*, and
the needle came back again to its former fituation, or
near it, in the night, or by the next morning. The
diurnal variation is *irregular*, when the needle moves
flowly eaftward in the latter part of the morning, or
weftward in the latter part of the afternoon; alfo
when it moves much either way after night, or fud-
denly both ways within a fhort time. Thefe irregu-
larities feldom happen more than once or twice in a
month, and are always accompanied (fo far as I have
been able to obferve) with an aurora borealis. Thus
having explained what I mean by the regular and ir-
regular diurnal variation, and fhewn, that this varia-
tion is generally regular; I fhall now, in the firft
place, endeavour to account experimentally for the
regular variation; then offer a conjecture concerning
the

the caufe of the irregular variation; and, laftly, attempt to make it appear probable, that the aurora borealis arifes from the fame caufe.

The attractive power of the magnet, (whether natural, or artificial) will *decreafe* while the magnet is *heating*, and *increafe* while it is *cooling*; as will appear by the following experiments.

Experiment 1. About E. N. E. from a compafs, a little more than three inches in diameter, I placed a fmall magnet two inches long, half an inch broad, and three-twentieths of an inch thick, parallel to the magnetic meridian; and at fuch a diftance, that the power of the fouth end of the magnet was but juft fufficient to keep the north end of the needle to the N. E. point, or to 45 degrees. The magnet being covered by a brafs weight of fixteen ounces, about two ounces of boiling water was poured into it, by which means the magnet was gradually heating for feven or eight minutes; and during that time, the needle moved about three quarters of a degree weftward, and became ftationary at 44° ¼; in nine minutes more, it came back a quarter of a degree, or to 44° ½; but was fome hours before it gained its former fituation, and ftood at 45°. *N. B.* The greater the power of the fame magnet, the more it will lofe in a given degree of heat.

Exp. 2. On each fide of the compafs, and parallel to the magnetic meridian, I placed a ftrong magnet of the fize above-mentioned; fo that the fouth ends of both the magnets acted equally on the north end of the needle, and kept it in the magnetic meridian; but if either of the magnets was removed, the needle was attracted by the other, fo as to ftand at 45 degrees.

grees. The magnets were both covered with brafs weights of fixteen ounces each. Into the eaftern weight I poured about two ounces of boiling water; and the needle in one minute moved half a degree, and continued moving weftward for about feven minutes, when it arrived at $2^\circ \frac{3}{4}$. It was then ftationary for fome time; but, in twenty-four minutes from the beginning, it came back to $2^\circ \frac{1}{2}$, and in fifty minutes to $2^\circ \frac{1}{4}$. I then filled the weftern weight with boiling water, and in one minute, the needle came back to $1^\circ \frac{1}{4}$; in fix minutes more, it ftood half a degree eaftward; and after that, in about forty minutes, it returned to the magnetic north, or its firft fituation.

It is evident, that the magnetic parts of the earth in the north on the eaft fide, and the magnetic parts of the earth in the north on the weft fide of the magnetic meridian, equally attract the north end of the needle. If then the eaftern magnetic parts are heated fafter by the fun in the morning, than the weftern, the needle will move weftward, and the abfolute variation will increafe; when the attracting parts of the earth on each fide the magnetic meridian have their heat increafing equally, the needle will be ftationary, and the abfolute variation will then be greateft; but, when the weftern magnetic parts are either heating fafter, or cooling flower than the eaftern, the needle will move eaftward, or the abfolute variation will decreafe; and when the eaftern and weftern magnetic parts are cooling equally faft, the needle will again be ftationary, and the abfolute variation will then be leaft. This may be ftill further illuftrated, by placing the compafs and two magnets, as in the laft experi-

ment, behind a fcreen near the middle of the day in fummer; then, if the fcreen be fo moved, that the fun may fhine only on the eaftern magnet, the needle will fenfibly vary in its direction, and move towards the weft; and if the eaftern magnet be fhaded, while the fun fhines on the weftern, the needle will move the contrary way. By this theory, the diurnal variation in the fummer ought to exceed that in the winter; and I accordingly find by obfervation, that the diurnal variation in the months of June and July, is almoft double that of December and January.

The irregular diurnal variation muft arife from fome other caufe than that of heat communicated by the fun; and here I muft have recourfe to fubterranean heat, which is generated without any regularity as to time, and which will, when it happens in the north, affect the attractive power of the magnetic parts of the earth on the north end of the needle. The reverend Dr. Hales has a good obfervation on this heat, in the Appendix to the fecond volume of his Statical Effays, which I fhall here tranfcribe. " That the warmth of " the earth, at fome depth under-ground, has an " influence in promoting a thaw, as well as the " change of the weather from a freezing to a thaw- " ing ftate, is manifeft from this obfervation; viz. " Nov. 29, 1731, a little fnow having fallen in the " night, it was, by eleven the next morning, moftly " melted away on the furface of the earth, except in " feveral places in *Bufhy-Park*, where there were " drains dug, and covered with earth, where the " fnow continued to lie, whether thofe drains were " full of water, or dry; as alfo where elm-pipes lay " under-ground; a plain proof that thefe drains inter-

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" cepted

" cepted the warmth of the earth from afcending
" from greater depths below them; for the fnow lay
" where the drain had more than four feet depth of
" earth over it. It continued alfo to lie on thatch,
" tiles, and the tops of walls."

That the air neareft the earth will be moft warmed
by the heat of it, is obvious; and this has frequently
been taken notice of in the morning, before day, by
means of thermometers at different diftances from
the ground, by the reverend Dr. Miles, at Tooting in
Surrey; and is mentioned in p. 526, of the 48th vo-
lume of the Philofophical Tranfactions.

The aurora borealis, which happens at the time
the needle is difturbed by the heat of the earth, is
fuppofed to be the electricity of the heated air above
it; and this will appear chiefly in the northern re-
gions, as the alteration in the heat of the air in thofe
parts will be greateft. This hypothefis will not feem
improbable, if it be confidered, that electricity is now
known to be the caufe of thunder and lightning; that
it has been extracted from the air at the time of an
aurora borealis; that the inhabitants of the northern
countries obferve the aurora to be remarkably ftrong,
when a fudden thaw happens after fevere cold wea-
ther; and that the curious in thefe matters, are now
acquainted with a fubftance, that will, without fric-
tion, both emit and abforb the electrical fluid, only
by the increafe, or diminution of its heat: for if the
Tourmalin be placed on a plane piece of heated glafs,
or metal, fo that each fide of it, by being perpendi-
cular to the furface of the heating body, may be
equally heated; it will, while heating, have the elec-
tricity of one of its fides pofitive, and that of the other

nega-

negative; this will likewise be the cafe when it is taken out of boiling water, and fuffered to cool; but the fide that was pofitive while it was heating, will be negative while it is cooling, and the fide that was negative, will be pofitive *.

For the fake of thofe who may be defirous of examining the diurnal variations of the needle very minutely, I fhall annex a complete year's obfervations; and fhall deduce, from the regular variations during that time, the mean diurnal variation belonging to each month: whence it will appear, that the diurnal variation increafes from January to June, and decreafes from June to December.

In the following table, the firft column contains the day of the month; and the fecond, the hour and minute of the day (according to equal time), when each obfervation was made. The third column contains the abfolute variation of the needle weftward, or the angle it made with a true meridian line at the refpective times; which differs lefs than two minutes of a degree from that at Greenwich, where the reverend Dr. Bradley was fo kind as to give me feveral opportunities of taking it, in his prefence, by the curious apparatus for that purpofe, belonging to the royal obfervatory. And in the fourth column are fet down the degrees of heat, by a Fahrenheit's thermometer, hung without, in the fhaded air, on the north fide of the houfe.

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* See the Gentleman's Magazine for laft September.

Spital

Spital Square, London, 1759.

D.	H.		M.	°	′	Th.	D.	H.		M.	°	′	Th.
1	1	M	0	18	54	34	6	0	A	5	19	6	39
	10		15	18	56	37		1		10	19	6	40
	11		0	18	57	38		1		30	19	5	40
	0	A	15	18	59	40		2		30	19	4	39
	1		0	19	0	42		9		10	18	54	33½
	1		45	18	59	42	7	11	M	0	18	59	42
	8		0	18	54	40		0	A	30	19	0	44
2	10	M	30	18	56	45		1		0	19	1	45
	1	A	12	18	59	46		1		30	19	0	47
3	0	M	10	18	54	45		2		30	18	59	46
	10		0	18	57	45		8		50	18	55	44
	0	A	20	19	2	44½	8	0	M	40	18	53	45
	1		0	19	3	44		8		45	18	53	47
	1		30	19	4	44		9		45	18	55	47½
	2		0	19	3	44		1	A	25	19	1	50
	4		0	18	59	45		4		50	18	56	48
	9		0	18	54	46		9		10	18	52	47
4	0	M	18	18	54	47	9	0	M	25	18	54	46
	9		30	18	56	49		8		53	18	57	44
	11		58	18	58	52		0	A	30	19	0	46
	1	A	55	18	58	53		1		15	19	0	47
	3		12	18	58	52½		9		0	18	57	49
5	0	M	30	18	54	51		11		30	19	0	52
	10		0	18	57	47		11		40	19	2	52
	0	A	30	19	0	46	10	9	M	0	18	55	52
	1		15	19	1	45½		10		10	18	56	52½
	2		20	19	0	45		1	A	0	18	59	54
	4		0	18	57	44		1		30	18	59	54½
	9		30	18	54	42		2		20	18	59	54
6	0	M	20	18	54	40		3		0	18	58	54
	10		15	19	2	37		9		10	18	55	53½
	10		45	19	4	38		11		45	18	55	52

January. | January.

D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
				January.						January.	
11	8 M	40	18	54	48	15	3 A	50	18	58	51
	9	40	18	55	49		9	10	18	55	50
	10	40	18	58	49	16	0 M	25	18	55	50
	11	50	19	0	49½		8	20	18	55	48
	0 A	40	19	2	51		9	20	18	55	49
	1	20	19	2	51		10	30	18	57	49½
	2	10	19	1	51		11	30	18	59	50
	2	45	19	0	51		1 A	0	19	2	50
12	0 M	15	18	57	47		2	55	19	0	49
	9	0	18	54	48		5	50	18	59	48½
	10	10	18	56	48		9	0	18	58	48
	11	5	18	58	48½	17	0 M	10	18	54	47
	0 A	5	19	0	49½		9	0	18	55	44
	1	20	19	1	50		10	15	18	56	45
	2	30	19	0	50		0 A	20	18	59	46
	5	15	18	57	49½		1	10	19	0	46½
	8	30	18	56	48		2	45	18	59	46½
13	0 M	30	18	56	46		6	0	19	1	46½
	9	20	18	55	47		9	0	18	48	46½
	10	20	18	56	47		9	5	18	45	46½
	11	50	18	58	48		9	20	18	46	46½
	1 A	15	19	0	45		9	30	18	47	46½
	2	30	18	59	44	18	0 M	20	18	50	46
	7	0	18	57	42		8	30	18	54	48½
14	0 M	10	18	58	43		10	0	18	55	48½
	9	45	18	56	46		11	30	18	58	49
	10	45	18	57	50		0 A	10	19	0	49½
	9 A	10	18	55	48		1	55	19	1	50
15	0 M	10	18	55	47		5	20	18	59	47
	8	30	18	55	48	19	0 M	5	18	55	43
	10	20	18	56	51		8	45	18	56	37
	11	35	18	58	52		11	0	18	59	39
	0 A	45	19	0	52½		0 A	20	19	1	39
	1	35	19	0	52		0	45	19	3	39½

D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
		January.						January.			
19	1 A	20	19	4	39	22	2 A	45	19	2	36½
	2	15	19	3	39		3	45	19	0	35
	3	35	19	1	38		8	0	18	58	32
	6	15	19	4	37		9	10	18	57	34
	6	32	19	0	37	23	0 M	10	18	59	31
	6	53	19	0	36		8	50	18	55	28
	9	0	18	59	34		9	50	18	56	29
20	0 M	5	18	57	32½	11	25		18	59	33	
	8	35	18	56	32½		1 A	10	19	2	37
	9	45	18	58	34		3	45	19	3	36½
	0 A	45	19	3	38		5	10	19	0	35
	1	25	19	4	40		7	0	18	59	34
	2	10	19	2	39½		9	0	18	56	33
	7	15	18	58	37	24	0 M	25	18	56	32
	8	45	18	54	36		9	0	18	57	35
	9	20	18	54	35		10	0	18	59	38
21	0 M	10	18	58	33		11	45	19	1	41
	10	0	18	57	32		0 A	15	19	2	42
	11	0	18	59	33		0	40	19	3	42
	12	0	19	1	35		1	15	19	4	42
	1 A	0	19	2	37		2	0	19	4	42½
	1	35	19	2	37½		3	0	19	4	43
	2	45	19	1	36½		3	55	19	2	41
	5	5	19	0	34		6	25	18	59	38
	9	55	18	57	31	25	0 M	25	18	50	38
22	0 M	30	18	55	28½		9	0	18	54	38
	8	30	18	54	28		11	0	18	58	43
	9	30	18	55	29		0 A	20	19	0	44
	10	40	18	58	32		1	20	19	1	45
	11	40	19	0	35		1	45	19	1	45
	0 A	30	19	2	36½		2	20	19	1	45
	1	10	19	4	37		4	0	18	59	44½
	1	35	19	4	37		7	45	18	57	44
	2	10	19	3	37	26	0 M	20	18	56	42

January.

D.	H.	M.	°	′	Th.
26	8 M	35	18	55	44
	10	40	18	56	46
	0 A	30	19	1	47
	1	25	19	4	47½
	2	20	19	3	47
	4	30	19	0	46
	9	0	18	57	45
	11	45	18	56	44
27	8 M	40	18	53	43
	10	0	18	54	44
	11	30	18	58	46
	1 A	7	19	0	48
	1	30	19	0	49
	2	10	19	1	49
	3	35	18	59	48
	8	20	18	57	47
	11	55	18	56	47
28	10 M	0	18	55	49
	11	0	18	56	49
	1 A	0	19	1	49
	1	30	19	2	49½
	2	40	19	1	47
	6	45	18	59	43
	8	50	18	56	42
29	0 M	25	18	56	39
	8	40	18	54	35
	10	15	18	55	38
	11	40	18	58	42
	1 A	0	19	2	45
	1	25	19	2	45
	2	0	19	3	45
	2	45	19	2	44
	3	35	19	0	43½
	9	0	18	58	42

January.

D.	H.	M.	°	′	Th.
30	0 M	25	18	57	41
	9	0	18	56	44
	10	0	18	54	46
	11	10	18	57	47
	1 A	0	19	3	48½
	6	30	18	58	47½
	11	55	18	54	47
31	8 M	30	19	0	45
	10	0	18	57	46
	11	0	18	58	47
	12	0	18	59	48
	0 A	58	19	1	49
	1	50	19	3	49
	2	15	19	2	49
	4	0	18	59	48½
	7	10	18	58	47
	11	55	18	57	46

February.

D.	H.	M.	°	′	Th.
1	8 M	40	18	54	39
	9	20	18	54	40
	10	0	18	55	41
	11	45	18	58	43
	0 A	50	19	0	45
	1	30	19	0	46
	2	12	18	59	46
	4	40	18	57	44
	11	40	18	56	40
2	9 M	15	18	55	46
	0 A	10	19	0	49
	1	15	19	1	50
	2	10	19	3	49½

Febru-

February.					
D.	H.	M.	°	′	Th.
2	2 A	40	19	2	49
	4	30	18	59	48½
	9	20	18	57	48
3	0 M	23	18	56	48
	8	30	18	57	49
	9	30	18	57	49
	10	15	18	58	49½
	0 A	30	19	6	50
	1	5	19	7	50
	1	40	19	8	49
	9	0	18	51	43
4	0 M	10	18	58	40
	9	30	18	54	42
	11	0	18	58	45
	11	20	18	24	45½
	11	22	18	16	45½
	11	22½	18	12	45½
	11	24	18	19	45½
	11	25½	18	14	45½
	11	27	18	21	45½
	11	29	18	34	45½
	11	30	18	29	45½
	11	31½	18	29	45½
	11	32½	18	24	45½
	11	33	18	19	45½
	11	35	18	24	45½
	11	36	18	24	45½
	11	38½	18	21	45½
	11	42	18	16	45½
	11	46	18	34	46
	11	48½	18	36	46
	11	53	18	45	46
	12	0	18	51	46
	0 A	6	18	54	46

February.					
D.	H.	M.	°	′	Th.
	0 A	9	18	57	46
	0	18	19	1	46½
	0	25	19	6	46½
	0	31	19	8	47
	2	0	19	1	48
	3	0	18	36	47½
	4	25	18	39	46½
	4	26½	18	54	46½
	4	28	19	4	46½
	4	29½	19	44	46½
	4	30½	19	46	46½
	4	32½	19	49	46½
	4	34	19	4	46½
	4	35½	19	19	46½
	4	36½	19	24	46½
	4	38	19	34	46½
	4	38½	20	4	46½
	4	39⅔	20	54	46½
	4	41	21	24	46½
	4	42¾	20	44	46½
	4	44⅓	19	24	46½
	4	45⅓	17	54	46½
	4	47	18	14	46½
	4	48½	18	32	46½
	4	51¾	17	29	46½
	4	52¾	17	54	46½
	4	54	18	24	46½
	5	7½	17	54	46
	5	9½	17	50	46
	5	39½	18	37	46
	5	51	19	9	45¾
	6	44	19	2	44
	6	53	18	57	43½
	8	40	19	2	42

A strong Aurora Borealis.

D.	H.	M.	°	′	Th.
4	10 A	10	18	59	41
5	8 M	32	19	11	43
	11	8	18	59	45
	11	58	19	2	47
	0 A	30	18	58	49
	1	40	19	4	50
	6	0	18	55	48
	6	42	18	49	48
	7	2¼	18	44	48
	7	28	18	46	48
	8	50	18	54	48
6	0 M	14	18	55	48½
	8	30	18	55	48
	10	0	18	55	48½
	11	55	19	1	49
	1 A	5	19	5	50
	2	45	19	4	49
	5	20	18	57	47
	8	0	18	56	44
	11	50	18	50	42
7	8 M	40	18	55	41
	9	50	18	56	43
	11	20	19	0	45
	1 A	17	19	4	49
	2	15	19	6	49
	6	30	19	1	48
	11	55	18	57	47
8	8 M	40	18	56	46
	10	10	18	56	48
	11	10	18	57	48½
	0 A	30	19	2	49
	1	40	19	3	49
	2	20	19	3	48½
	3	55	19	2	48

An Aurora Bor. (left margin, February)

D.	H.	M.	°	′	Th.
8	9 A	25	19	1	46
9	0 M	10	18	59	46
	8	55	18	58	45
	10	0	18	58	45½
	11	40	19	0	46
	0 A	55	19	4	46½
	1	50	19	5	47
	2	15	19	4	47
	5	35	19	0	45
	9	15	18	58	43
10	0 M	50	18	57	42
	8	35	18	56	41
	10	0	18	56	43
	11	30	18	59	44
	1 A	15	19	4	45
	2	10	19	6	44
	8	30	19	2	41
11	1 M	10	18	58	42
	10	0	18	58	47
	11	0	18	59	49
	0 A	5	19	1	51
	1	30	19	3	51½
	2	30	19	4	51
	4	20	19	0	47
	7	30	18	59	44
	10	5	18	58	43
12	0 M	45	18	59	42
	8	40	18	55	41
	10	0	18	55	43
	11	0	18	56	45
	12	0	18	59	48
	1 A	0	19	2	49
	2	0	19	4	49
	2	45	19	3	48

D.	H.	M.	°	′	Th.
12	3 A	45	19	3	47
	6	20	18	58	43
	9	0	18	57	42
	11	50	18	55	40
13	1 M	0	18	55	38
	8	50	19	5	37
	9	57	19	2	39
	10	50	19	4	43
	0 A	35	19	3	48
	1	10	19	3	49
	3	20	19	4	48
	5	30	18	59	43
	6	30	18	59	42
	9	10	18	57	41
	11	38	18	57	41
14	8 M	50	18	55	40
	10	10	18	55	40
	11	50	18	58	40½
	2 A	40	19	2	42
	7	25	18	58	38
15	0 M	30	18	57	34
	8	55	18	55	34½
	9	55	18	55	37
	11	0	18	57	39
	0 A	20	19	1	41
	1	40	19	5	43
	3	15	19	4	42
16	0 M	5	18	57	40
	9	40	18	55	40
	11	45	18	58	42
	1 A	0	19	0	43
	2	15	19	4	43
	5	45	19	0	42
	9	5	18	56	41

D.	H.	M.	°	′	Th.
17	0 M	5	18	56	40
	8	45	18	54	37
	9	50	18	55	38
	11	0	18	57	40
	0 A	5	19	0	43
	1	15	19	2	46
	2	10	19	2	43
	3	12	19	1	46
	7	40	18	59	40
18	0 M	30	18	57	35
	9	55	18	57	40
	11	5	18	59	43
	0 A	10	19	1	46
	1	5	19	2	48
	2	15	19	2	47
	4	30	19	0	45
	9	45	18	58	41
19	0 M	30	18	58	42
	8	35	18	56	40
	10	0	18	56	40½
	11	0	18	58	41
	0 A	15	19	1	42
	1	35	19	4	44
	2	10	19	5	43
	3	0	19	5	42
	4	0	19	4	41
	7	10	19	1	40
	9	4	18	47	38½
	9	5	18	43	38½
	9	7	18	43	38½
	9	25	18	31	38
	9	27	18	29	38
	11	40	18	47	38
20	8 M	50	18	54	39

Febru-

February.					
D.	H.	M.	°	′	Th.
20	9 M	55	18	54	40
	11	0	18	56	41
	0 A	25	19	1	41½
	1	15	19	4	42
	2	30	19	4	41½
	6	20	19	5	41
	9	25	18	53	40
21	1 M	20	18	56	40
	8	50	18	54	39
	10	0	18	55	39
	12	0	18	58	39
	1 A	40	19	2	42
	2	30	19	1	41½
	4	30	18	59	38
	8	0	18	57	36
22	0 M	55	18	58	39
	9	5	18	55	40
	11	0	18	56	44
	1 A	0	19	0	48
	2	5	19	3	47½
	3	10	19	2	47
	5	15	18	59	46½
	9	25	18	58	46
23	0 M	25	18	58	45
	8	50	18	55	43
	9	55	18	55	44
	11	5	18	57	44
	0 A	13	19	1	43
	1	42	19	4	43
	3	8	19	3	43
	5	22	18	59	43
	9	40	18	58	40
24	0 M	50	18	59	38
	10	0	18	55	44

February.					
D.	H.	M.	°	′	Th.
24	11 M	15	18	56	45
	1 A	40	19	3	40
	2	40	19	4	40
	3	20	19	2	40
	4	0	19	1	39
	7	5	18	59	39
	9	15	18	49	40
	11	58	18	57	40
25	10 M	0	18	55	40½
	0 A	28	18	59	41
	1	55	19	3	41
	3	20	19	2	41
	4	20	19	0	42
	6	25	18	59	42
	9	55	18	59	43
26	9 M	0	18	53	46
	10	0	18	53	48
	11	5	18	55	50
	0 A	18	19	0	52
	1	30	19	3	54
	2	22	19	3	53
	4	5	19	2	50
	8	0	18	58	49
	11	53	18	58	48
27	9 M	23	18	59	49
	10	10	19	1	50
	11	25	19	2	51
	1 A	25	19	9	52
	2	15	19	10	52½
	2	58	19	4	53
	3	56	19	3	52
	5	50	19	3	51
	8	30	18	54	50
	9	8	18	52	49

Febru-

		February.			
D.	H.	M.	°	′	Th.
28	0 M	18	18	54	46
	10	25	18	53	43
	11	25	18	57	45
	0 A	25	19	0	47
	1	10	19	3	49
	1	57	19	4	48
	7	30	18	59	46
	9	5	18	59	45
	11	45	18	58	44

		March.			
D.	H.	M.	°	′	Th.
1	8 M	48	18	58	45
	10	7	18	57	48
	11	10	18	59	50
	0 A	38	19	3	50½
	1	50	19	4	51
	2	45	19	3	51
	4	7	19	0	47
	9	0	18	58	45
	11	26	18	57	43
2	8 M	35	18	55	44
	9	55	18	56	45
	11	15	18	58	47
	0 A	25	19	1	49
	1	40	19	3	51
	2	10	19	3	51
	3	30	19	1	49
	4	50	19	0	47
	8	35	18	59	46
3	0 M	23	18	57	43
	8	20	18	54	40
	10	0	18	55	43

		March.			
D.	H.	M.	°	′	Th.
3	11 M	5	18	57	45
	0 A	20	19	3	46½
	1	7	19	7	48
	2	0	19	4	49
	3	5	19	2	48
	8	20	18	58	43½
	11	55	18	54	41½
4	9 M	30	18	54	42½
	11	0	18	56	44
	0 A	10	19	1	46
	1	43	19	4	45½
	2	40	19	4	46
	4	30	19	2	45½
	6	50	18	59	43½
	10	10	18	57	42½
5	0 M	55	18	57	43
	9	0	18	55	42½
	11	10	18	59	45½
	0 A	28	19	4	46½
	1	50	19	5	47
	2	40	19	6	46
	3	56	19	4	44
	7	15	19	0	42½
6	0 M	45	18	58	42
	8	40	18	56	42
	9	30	18	56	43
	11	23	19	0	43½
	0 A	10	19	2	43½
	1	25	19	4	43
	2	0	19	5	42½
	4	0	19	3	42
	6	20	19	0	42½
	9	40	18	58	43
	11	53	18	57	40¼

		March.						March.			
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
7	9 M	15	19	16	42	10	2 A	45	19	2	44
	9	50	19	12	43		3	35	19	1	43½
	11	25	19	3	45		4	32	19	0	44½
	0 A	19	19	12	46		7	0	18	59	41
	0	43	19	8	47		11	47	18	59	36
	1	10	19	4	46½	11	10 M	10	18	58	42½
	2	15	19	3	47½		11	7	19	0	43
	3	0	19	5	45½		12	0	19	1	43
	5	10	19	2	42½		1 A	15	19	2	44½
	7	50	18	59	40		2	35	19	1	44
8	0 M	10	18	57	39		7	32	18	59	40
	8	38	18	58	40	12	0 M	50	18	59	36
	10	0	18	57	43		8	47	18	57	37½
	11	20	18	59	45		10	30	18	59	43
	0 A	15	19	0	46		0 A	28	19	4	47
	1	26	19	1	47		1	55	19	5	49½
	2	20	19	0	46½		3	15	19	3	47½
	4	40	18	58	43		5	5	19	1	46½
	6	30	18	58	42	13	0 M	22	18	59	40
9	0 M	5	18	54	43		8	56	18	57	42
	8	30	18	57	40		10	0	18	59	45
	9	55	18	55	41		11	20	19	3	48½
	11	55	19	2	45		0 A	25	19	6	49½
	1 A	12	19	4	48		1	5	19	7	51
	2	5	19	4	49		1	28	19	7	52
	4	7	18	59	48½		6	45	18	59	46
	6	35	18	58	46	14	1 M	17	18	59	42
	8	50	18	59	46		8	45	18	56	48
	11	30	19	0	44½		10	0	18	57	51
10	9 M	20	18	58	44		11	20	18	59	52
	10	15	18	59	44		0 A	52	19	4	53
	11	30	19	2	42		2	20	19	5	53
	0 A	52	19	3	42		3	5	19	3	53
	1	30	19	4	42½		5	35	19	0	51

	March.				
D.	H.	M.	°	′	Th.
14	9 A	5	18	59	50
15	0 M	7	18	59	49½
	8	30	18	55	48
	10	10	18	54	49
	11	5	18	56	50
	0 A	20	19	0	51
	1	42	19	1	51
	2	35	19	1	51
	3	30	19	0	50½
	11	55	18	57	50
16	8 M	30	18	53	50½
	10	0	18	54	51
	11	10	18	58	52
	0 A	16	19	2	53
	1	15	19	4	49
	2	20	19	5	47
	3	55	19	1	47½
	6	20	18	59	45
	9	5	18	58	43
17	0 M	35	18	59	40
	8	5	18	54	39
	9	10	18	53	43
	10	15	18	55	45½
	11	20	19	0	48
	0 A	25	19	4	49
	1	35	19	7	44
	4	0	19	2	46
	8	45	18	58	40½
18	0 M	10	18	57	39½
	9	50	18	55	43
	11	0	18	59	44
	4 A	56	19	7	40
	6	37	18	54	39½
19	0 M	10	18	55	37

	March.				
D.	H.	M.	°	′	Th.
19	8 M	40	18	54	37
	10	15	18	56	42
	11	20	19	0	43½
	0 A	45	19	7	43½
	1	22	19	14	42½
	1	58	19	13	42
	2	18	19	11	41½
	3	10	19	9	41
	4	0	19	8	41
	5	15	18	58	41½
	5	45	18	54	41½
	6	33	18	58	43
	7	17	19	0	44
	11	55	18	46	46
20	7 M	55	18	54	45½
	8	46	18	54	47
	9	46	18	56	48½
	10	46	18	58	49
	11	46	19	2	49
	1 A	0	19	11	48½
	1	46	19	9	48
	2	58	19	8	49
	3	59	19	6	47½
	5	0	19	2	47
	6	0	19	0	45
	8	30	18	55	44½
21	1 M	12	19	4	39½
	8	55	18	56	40½
	10	0	18	55	42½
	11	0	18	59	42½
	0 A	10	19	3	43½
	1	5	19	5	44½
	1	50	19	6	46
	2	50	19	4	45½

	March.						March.				
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
21	4 A	0	19	1	45	24	9 A	5	19	0	45
	5	30	18	58	43	25	0 M	35	19	1	41
	8	5	18	57	40		10	0	18	58	45½
	11	50	18	57	37½		11	0	19	0	46½
22	9 M	0	18	54	44		0 A	5	19	2	48
	10	0	18	56	46		1	45	19	4	52
	11	0	19	0	48½		2	30	19	3	51½
	12	0	19	3	52½		4	35	19	1	51
	1 A	0	19	6	52		6	43	19	0	49
	1	30	19	6	52½		9	38	18	59	48
	2	0	19	6	53	26	0 M	15	18	59	47½
	3	0	19	6	54		8	30	18	55	47
	4	30	19	8	52		9	20	18	56	48
	7	53	18	48	48		10	5	18	58	49
	9	0	18	26	48½		11	10	19	3	50
23	0 M	50	18	53	44		0 A	5	19	6	51
	9	0	18	59	48		1	12	19	7	51½
	10	0	19	0	50		1	45	19	6	52
	11	0	19	2	53		3	0	19	2	52½
	12	0	19	4	54½		3	50	19	0	52½
	1 A	0	19	5	55		6	56	18	59	49
	2	0	19	5	56		9	2	18	59	46½
	3	0	19	4	57	27	0 M	5	18	59	44
	4	0	19	2	56		8	12	18	53	48
	5	0	19	1	54		9	5	18	52	50
	9	0	18	59	50		10	4	18	56	51½
24	0 M	38	18	58	47		11	15	19	2	53½
	8	54	18	54	50		0 A	10	19	5	55
	10	5	18	56	50		1	15	19	7	57
	11	3	19	0	50½		2	50	19	6	56½
	0 A	2	19	4	52		4	0	19	3	57
	1	5	19	5	51		5	0	18	59	56
	2	2	19	7	51½		6	25	18	59	54
	3	1	19	4	52½		9	40	18	58	50

An Aurora Bor.

Janu-

		March.			
D.	H.	M.	°	′	Th.
28	0 M	40	18	58	49
	8	15	18	52	49
	9	15	18	54	49½
	10	0	18	55	50½
	0 A	2	19	3	51
	1	3	19	7	52
	1	47	19	8	52½
	3	12	19	6	52½
	4	20	19	3	52
	5	5	19	1	52
	8	0	18	59	50
	9	10	18	54	49
29	0 M	15	18	58	47
	8	10	18	50	49
	9	10	18	51	48½
	10	10	18	54	51
	11	20	19	1	52
	0 A	32	19	7	52
	1	45	19	9	52
	2	15	19	9	52
	3	4	19	6	51
	4	5	19	2	48½
30	0 M	10	18	59	40
	8	7	18	52	41
	9	5	18	52	41
	10	20	18	56	43
	11	15	19	0	45
	0 A	20	19	6	45½
	1	15	19	8	46
	1	57	19	8	46½
	3	10	19	6	45
	4	12	19	3	43
	5	30	19	1	42
	9	35	18	59	37

		March.			
D.	H.	M.	°	′	Th.
31	0 M	10	18	59	35½
	8	25	18	54	35
	9	15	18	53	36
	10	10	18	54	37½
	11	5	18	58	40
	0 A	10	19	5	42
	1	5	19	8	43
	1	35	19	9	43½
	2	22	19	7	43½
	4	10	19	3	43½
	8	10	19	0	40

		April.			
D.	H.	M.	°	′	Th.
1	0 M	40	18	59	37
	9	50	18	55	50½
	11	0	18	58	55
	0 A	2	19	2	55
	1	30	19	6	54½
	2	0	19	6	55
	3	2	19	4	54
	5	15	19	0	54
	6	5	18	59	52½
	9	45	18	59	48
2	0 M	40	18	58	45
	7	50	18	53	44
	9	10	18	54	48½
	10	0	18	56	51
	0 A	10	19	3	55
	1	3	19	5	57
	1	0	19	6	56½
	2	10	19	6	56
	3	0	19	5	55

D.	H.	M.	°	′	Th.
2	4 A	0	19	2	53½
	4	56	19	1	53
	7	20	18	59	47
3	0 M	35	18	58	41
	8	10	18	54	43
	9	15	18	54	48½
	10	0	18	56	51
	11	4	19	0	53
	0 A	2	19	4	55
	1	0	19	7	56
	1	20	19	7	56½
	4	11	19	2	56
	6	15	18	58	52
	8	43	18	58	46
4	0 M	40	18	58	42
	8	15	18	51	47
	9	10	18	51	48
	10	6	18	54	50
	11	7	19	0	52½
	12	0	19	5	50½
	1 A	0	19	6	52
	1	30	19	7	53
	2	8	19	7	54
	3	13	19	4	54½
	4	20	19	1	54
	5	20	19	0	53
	8	45	18	54	50
5	0 M	5	18	53	49½
	8	10	19	24	51
	9	8	19	18	53
	10	15	19	9	56
	11	10	19	10	57½
	0 A	7	19	15	58
	1	0	19	15	58

D.	H.	M.	°	′	Th.
5	1 A	30	19	16	58
	2	20	19	19	57
	4	12	19	12	56
	7	30	19	1	52
6	0 M	11	19	1	48
	8	10	18	54	50
	9	9	18	55	50½
	10	12	18	58	51
	11	25	19	3	51
	0 A	10	19	4	49½
	1	30	19	5	47
	2	15	19	4	46
	3	18	19	2	45
	5	18	18	58	46½
7	1 M	27	18	58	42
	8	25	18	53	43½
	9	16	18	54	46
	10	20	18	57	49½
	11	5	18	59	51
	0 A	16	19	4	50½
	1	0	19	5	51
	2	1	19	5	51
	2	40	19	3	52
	9	1	18	59	44½
8	0 M	25	18	59	42½
	9	48	18	58	47
	11	3	19	0	50
	0 A	10	19	3	51
	1	45	19	5	49
	2	35	19	4	49
	4	10	19	2	46
	7	46	19	0	48
9	0 M	37	18	59	48
	9	9	18	54	51½

D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
9	10 M	5	18	56	54	12	2 A	0	19	7	54
	11	6	18	59	54½		3	1	19	5	54½
	0 A	5	19	4	57		9	5	19	0	48
	1	5	19	9	57	13	0 M	23	18	56	45
	1	40	19	8	56		9	30	18	57	50
	3	10	19	6	55		10	8	13	59	51½
	4	7	19	3	54½		11	7	19	3	53½
	6	45	19	0	53½		0 A	12	19	7	55
	11	40	18	59	46½		1	30	19	12	56
10	9 M	10	18	53	47		1	58	19	14	57
	10	1	18	55	48½		2	10	19	15	57
	11	3	18	59	49½		2	24	19	13	57
	0 A	2	19	1	51		3	10	19	8	57
	1	10	19	6	53		4	20	19	6	56
	1	43	19	7	53		5	35	19	4	54½
	5	55	19	1	50		6	20	19	2	53
	7	50	18	59	46		9	22	18	44	49
	10	0	18	57	44	14	0 M	57	18	53	44
11	0 M	55	18	57	42		9	0	18	52	48
	9	20	18	58	49		10	3	19	0	49
	10	8	19	0	51½		11	4	19	4	50
	11	15	19	4	51½		0 A	12	19	7	51
	0 A	10	19	6	51½		1	5	19	5	51½
	1	1	19	8	53		1	35	19	9	51½
	1	40	19	8	54		2	0	19	8	52
	2	10	19	7	54		3	0	19	2	51
	3	1	19	5	53		8	0	18	57	49
	11	40	19	0	47	15	0 M	14	18	57	45
12	8 M	10	18	55	47½		9	10	18	56	47
	9	10	18	56	49		10	11	18	56	47½
	10	3	18	58	50½		11	10	18	58	50
	11	2	19	3	51		0 A	3	19	1	50
	0 A	11	19	6	52½		1	25	19	5	52
	1	25	19	7	54		2	0	19	6	51½

	April.							April.				
D.	H.	M.	°	′	Th.		D.	H.	M.	°	′	Th.
15	5 A	12	19	1	50		19	1 A	40	19	7	51
	7	8	18	58	49			9	20	19	0	48
	9	55	18	58	44		20	0 M	17	19	0	47½
16	0 M	48	18	57	41			9	15	18	56	54
	9	5	18	58	41½			11	5	19	0	56
	10	0	19	0	42½			0 A	54	19	3	59
	11	10	19	5	44			1	38	19	4	59½
	0 A	5	19	6	45½			2	5	19	2	60½
	1	6	19	6	48			8	6	18	58	52
	2	0	19	6	48½			11	45	18	57	50½
	2	59	19	5	47		21	8 M	20	18	52	54
	4	0	19	2	47			9	10	18	54	55½
	5	5	19	2	46			10	5	18	56	56½
	10	2	18	32	42			11	20	18	59	59
17	1 M	20	18	55	40			0 A	10	19	4	62
	9	50	18	58	50			1	2	19	6	62
	11	1	19	2	50			1	42	19	7	63
	0 A	10	19	5	53			9	3	18	59	53
	1	30	19	8	53			11	50	18	58	49½
	6	0	19	2	49		22	8 M	35	18	55	55½
	9	8	18	58	43			9	15	18	55	56
18	0 M	35	18	59	41			11	5	18	56	60½
	8	30	18	56	46			0 A	2	19	1	60
	10	10	18	58	48½			1	16	19	4	63
	11	17	19	3	50½			2	10	19	4	62½
	1 A	0	19	7	50½			9	25	19	0	54
	1	40	19	8	50½		23	0 M	26	18	59	51
	2	15	19	7	50½			8	10	18	51	52
	3	10	19	5	49½			9	4	18	53	53½
	9	35	19	1	41			10	55	18	58	57
19	0 M	25	19	0	38			1 A	12	19	4	59½
	8	50	18	54	44½			1	40	19	4	60
	11	5	19	1	49			3	5	19	3	61
	1 A	0	19	7	50½			7	8	18	59	51

April.

D.	H.	M.	°	′	Th.
24	0 M	4	18	59	42
	7	45	18	55	45
	9	10	18	57	47
	10	55	19	2	49
	1 A	25	19	6	52
	3	15	19	5	51
	7	8	18	55	44
	9	3	18	52	41
25	0 M	8	18	46	40
	8	45	18	57	48
	9	20	19	3	52
	11	8	19	6	55
	0 A	55	19	8	56
	1	40	19	9	56
	2	15	19	8	56
	3	20	19	5	55
	5	12	18	59	54
	8	54	18	58	48½
26	0 M	10	18	57	46
	8	0	18	54	50
	9	20	19	0	53
	11	10	19	6	56
	0 A	45	19	9	57½
	1	25	19	10	58
	2	0	19	8	56
	4	55	19	0	57
	8	55	18	59	52
	11	42	18	58	50
27	8 M	5	18	50	55
	9	7	18	52	56
	11	4	19	0	60
	1 A	6	19	5	60
	2	8	19	4	59
	3	12	19	2	58

April.

D.	H.	M.	°	′	Th.
27	7 A	12	18	58	53
28	1 M	10	18	57	49
	8	0	18	50	50½
	9	15	18	53	51½
	10	56	19	2	53
	0 A	2	19	6	55
	1	8	19	6	55
	1	40	19	7	55½
	2	20	19	5	56
	3	20	19	3	55
	9	15	18	59	52
29	1 M	2	18	59	51
	9	0	18	54	54½
	11	2	18	58	58½
	0 A	55	19	4	60
	1	32	19	3	60½
	5	4	18	59	57
	9	26	18	59	47
	11	42	19	1	44
30	8 M	0	18	55	51½
	8	58	18	56	54
	11	6	19	3	61
	1 A	2	19	6	63½
	1	50	19	6	63½
	2	52	19	4	62
	4	7	19	2	60½
	5	15	19	0	58
	7	55	19	0	51

May.

D.	H.	M.	°	′	Th.
1	8 M	40	18	56	51
	10	56	19	0	57½

D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
			May.						May.		
1	1 A	30	19	5	60	5	2 A	50	19	7	56
	4	3	19	2	57½		5	0	19	2	54
	9	2	18	59	48½	6	0 M	25	19	1	46
	11	55	18	58	46		9	18	18	56	54
2	7 M	48	18	54	51		11	1	19	1	56
	9	7	18	54	55		0 A	10	19	3	58
	11	7	18	59	58		0	55	19	4	58½
	1 A	28	19	3	59		1	35	19	4	60
	2	13	19	2	58		7	15	18	59	55
	5	1	18	59	54	7	1 M	5	19	2	48
	8	0	18	58	48		8	10	18	49	54½
3	0 M	15	18	58	46		9	10	18	54	57
	8	7	18	52	53		11	5	19	3	60
	9	30	18	53	54½		0 A	15	19	6	63
	0 A	59	19	0	55½		1	2	19	8	63
	1	47	19	3	55½		1	35	19	7	64
	2	45	19	2	55½		3	4	19	4	62½
	4	10	19	0	56		5	0	19	0	61
	11	58	18	59	51½		7	14	18	58	58½
4	8 M	15	18	51	56		10	0	18	58	56
	9	16	18	52	58	8	0 M	20	19	1	54
	11	2	19	4	64		8	15	18	54	56
	0 A	20	19	6	64		9	8	18	57	57
	1	5	19	7	64½		11	2	19	2	60½
	1	55	19	7	63		1 A	4	19	4	61½
	3	0	19	8	61½		3	10	19	1	63½
	4	58	19	3	57		9	20	18	59	57
	9	30	18	39	53	9	0 M	2	18	58	55
5	0 M	21	18	46	53		8	30	18	58	59
	8	25	18	56	52		9	20	18	54	60½
	9	35	18	59	53	10	18	18	58	63	
	11	0	19	2	55		11	35	19	3	64½
	0 A	55	19	8	54		0 A	18	19	8	63½
	1	40	19	9	55½		1	30	19	7	65

D.	H.	M.	°	′	Th.
		May.			
9	3 A	8	19	3	64
	5	2	18	58	61
	9	5.	18	58	55
10	0 M	25	18	56	53
	8	5	19	3	55
	9	20	18	59	59
	11	15	19	2	59
	0 A	50	19	4	61½
	1	15	19	6	62
	1	40	19	6	62
	3	20	19	4	61½
	9	25	18	56	52½
11	0 M	15	18	52	51
	8	5	18	52	55
	9	25	18	53	57
	11	20	18	55	58½
	1 A	10	18	54	58
	1	27	19	0	57½
	1	37	19	2	57½
	2	15	18	47	57½
	3	0	18	58	59½
	6	3	18	56	58
	9	7	19	8	55
	9	8	18	58	55
	9	9½	18	48	55
	9	28½	18	53	55
	9	32½	18	46	55
12	0 M	50	18	51	52
	7	55	18	51	57
	9	20	18	53	59
	11	15	19	1	62
	0 A	40	19	2	66
	1	12	19	3	65
	3	0	19	3	65½

An Aurora Bor.

D.	H.	M.	°	′	Th.
		May.			
12	5 A	0	18	59	64
	9	40	18	58	58
13	0 M	3	18	58	57
	8	30	18	53	60½
	11	2	18	59	64
	0 A	45	19	3	64½
	1	30	19	12	64
	2	13	19	8	64½
	5	12	19	1	62
	9	58	18	55	57
14	0 M	52	18	56	55½
	8	10	18	54	58
	9	12	18	56	58½
	11	13	19	0	62
	1 A	15	19	4	65
	2	7	19	5	64
	3	12	19	3	63
	7	13	19	0	57½
	9	10	18	59	55
	11	45	19	0	54
15	8 M	0	1?	55	57½
	9	5	1?	54	58½
	11	10	19	2	62
	0 A	30	19	8	65
	1	20	19	9	66
	3	15	19	7	66
	7	10	19	0	61
16	0 M	30	19	1	54
	8	10	18	53	60
	9	15	18	53	64
	11	28	18	59	69½
	1 A	15	19	6	72
	2	0	19	5	72½
	3	20	19	0	72½

May.

D.	H.	M.	°	′	Th.
16	7 A	10	18	58	65
	8	40	18	58	62
17	0 M	50	18	57	56
	8	20	18	53	56
	11		19	2	61
	0 A	4	19	8	62
	2	3	19	6	63
		4	18	59	52
18	M		18	57	49
	8	1	18	56	52½
	9	15	18	57	53
	11		19		56
	1 A	20	19		57
	2	10	19		57½
	6	50	19		52
	9	0	19		47
19	1 M	0	19		41½
	8	0	18	5	50
	9	10	19	0	52
	11	10	19	6	55
	0 A	50	19		58
	1	40	19		58½
	2	20	19	6	59
	3	2	9	4	60
	5	20	9	1	54
20	0 M	10	9	0	44
	9	22	8	58	55
	10	26	9	2	7
	0 A	30	9	5	1½
	1	40	19	6	1½
	2	35	19	6	2
	9	15	19	2	
21	1 M	20	19	3	3
	8	15	18	56	53

May.

D.	H.	M.	°	′	Th.
21	9 M	15	18	57	56
	11	0	19	1	61½
	0 A	40	19	5	65
	1	24	19	7	66
	1	55	19	6	66
	6	3	19	1	65
	9	15	19	1	58
22	0 M	5	18	56	54
	8	15	18	52	56
	9	45	18	56	59
	11	2	19	2	59
	1 A	0	19	7	62
	1	30	19	6	61½
	3	25	19	2	62
	5	35	18	58	61
	9	20	19	0	54½
23	0 M	25	19	0	52
	10		18	52	57½
	11	15	19	2	66
	0 A	5	19	6	67
	0	55	19	7	68
	1	30	19	7	68
	2	5	19	8	67½
	6	55	18	57	58½
	9	15	18	58	54
	11	50	18	56	54½
24	8 M	48	18	55	56
	11	5	19	7	57½
	0 A	13	19	9	60
	1	7	19	11	63
	1	48	19	9	64
	3	6	19	7	66
	9	2	19	0	60½
25	0 M	40	18	58	54

D.	H.	M.	°	′	Th.
		May.			
25	8 M	20	18	56	58
	9	32	19	0	63
	11	15	19	8	70
	0 A	27	19	10	72
	1	20	19	9	72½
	3	10	19	6	73½
26	0 M	15	18	59	56
	8	5	18	54	60
	9	10	18	58	61½
	10	15	19	2	63
	12	0	19	8	64
	0 A	25	19	9	64½
	5	15	19	4	57
	6	50	19	1	53
	9	40	19	0	46
27	0 M	40	19	2	43
	7	55	18	53	50
	11 A	58	19	3	51
28	8 M	55	18	54	57½
	11	10	19	1	61
	0 A	35	19	6	63½
	1	17	19	7	63
	4	25	19	3	63
	9	12	19	1	55
29	0 M	15	19	0	52
	8	28	18	56	57½
	9	30	18	58	60½
	10	25	19	0	60
	11	0	19	3	59
	0 A	15	19	7	59
	1	15	19	9	57
	2	15	19	7	53
	3	10	19	5	53
	5	0	19	3	53½

D.	H.	M.	°	′	Th.
		May.			
29	9 A	30	18	56	47
	11	58	18	55	41
30	8 M	20	18	56	49
	11	7	18	59	53
	3 A	5	19	3	56
	9	15	19	1	49½
	11	50	18	57	46
31	8 M	45	18	54	54
	9	30	18	53	59
	3 A	55	19	0	63½
	9	5	18	55	56
		June.			
1	0 M	23	18	56	55
	9	5	18	51	59½
	11	0	19	0	61½
	0 A	20	19	1	62½
	1	10	19	1	63
	2	15	19	0	64
	9	30	18	59	54
2	0 M	15	18	56	52
	8	35	18	52	57
	11	12	19	0	63
	11	45	19	3	63½
	9 A	20	18	55	55
3	0 M	40	18	56	54½
	9	0	18	53	57
	11	14	18	58	56
	0 A	58	19	2	59½
	1	30	19	3	59
	3	2	19	1	60
	5	30	18	58	58

D.	H.	M	°	′	Th.	D.	H.	M	°	′	Th.
				June.						June.	
3	10 A	20	18	55	54	7	1 A	15	19	10	63½
4	0 M	50	18	54	53		11	50	18	59	53½
	9	15	18	51	57½	8	7 M	6	18	53	53
	11	15	18	56	58½		9	10	18	57	60
	1 A	30	19	1	58½		11	30	19	3	63
	2	20	18	57	58	9	0 M	15	19	0	54
	3	12	19	0	57½		8	50	18	54	60½
	4	15	19	2	58½		11	48	19	6	65
	4	45	19	4	58		1 A	0	19	8	66
	7	30	19	0	54		1	35	19	9	66½
	9	30	19	1	50		3	10	19	6	66½
5	0 M	13	19	0	48½		5	0	19	3	64
	8	50	18	54	58		9	10	19	1	56½
	0 A	15	19	4	57	10	0 M	30	19	1	52½
	1	0	19	7	57		9	0	18	54	68
	1	47	19	8	58½		11	1	19	0	71½
	9	0	19	2	54½		1 A	12	19	6	72½
6	0 M	15	19	1	53		1	37	19	8	73½
	8	50	18	56	57		2	1	19	7	73½
	10	12	18	59	56		5	30	19	1	66
	11	15	19	2	57		7	15	19	0	63
	0 A	10	19	4	57		10	0	19	0	58
	1	12	19	7	58	11	0 M	52	19	0	57
	1	35	19	8	58		8	5	18	53	59
	3	0	19	..	60		9	4	18	53	61
	5	20	19	1	60		10	2	18	56	62
	7	15	18	58	57½		11	20	18	59	65
	8	30	19	0	56		1 A	2	19	6	65
7	0 M	2	19	0	53		1	32	19	7	65½
	8	30	18	52	58		3	30	19	5	59
	9	0	18	53	59		9	2	19	1	53
	10	1	18	58	60½	12	0 M	25	19	1	53
	11	12	19	5	62		7	48	18	50	56
	0 A	45	19	10	63		9	5	18	54	57

June.						June.					
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
12	11 M	10	19	3	60¼	16	11 M	3	19	0	66½
	1 A	2	19	8	62½		4 A	55	19	0	6~
	5	15	19	2	60		9	2	18	59	59
	9	7	19	0	54½	17	0 M	40	18	57	55
13	8 M	6	18	57	59½		8	9	18	55	62
	9	10	18	57	61		9	0	18	56	62
	11	35	19	6	62½		10	24	18	58	67
	1 A	15	19	8	61½		11	15	19	2	68½
	1	50	19	7	62		0 A	13	19	3	69
	3	0	19	6	62½		1	0	19	5	69½
	9	33	19	2	53½		1	30	19	7	70½
14	0 M	15	19	3	52		5	3	19	3	71½
	8	7	18	56	59		7	0	18	57	68
	11	20	19	2	65½		9	27	18	59	64½
	1 A	0	19	6	67	18	0 M	45	19	0	62
	1	33	19	7	66		7	42	18	50	65½
	3	10	19	5	67		8	52	18	51	70
	9	20	18	58	56½		10	7	18	56	73
15	0 M	1	18	59	54		11	0	19	1	75
	7	45	18	58	59		12	0	19	4	75½
	8	56	18	56	62½		1 A	2	19	4	77
	10	15	18	59	64		1	40	19	5	76
	11	20	19	4	67		2	55	19	4	76½
	0 A	25	19	6	65		4	7	19	1	75
	1	0	19	7	65		5	35	19	0	74
	1	30	19	8	63½		7	20	18	59	71
	2	3	19	8	64		9	35	18	55	68
	3	7	19	5	62	19	0 M	15	18	56	64
	5	10	19	2	60		7	47	18	58	68
	7	30	18	59	59		9	5	18	58	72½
	9	28	18	56	58½		10	4	18	56	75
16	0 M	44	18	57	58½		11	20	19	0	77
	7	40	18	55	61½		0 A	20	19	5	79
	8	50	18	58	63½		1	14	19	4	78

June.

June.

D.	H.	M.	°	′	Th.
19	3 A	20	19	3	78½
	5	10	19	0	76
	6	8	18	57	74
	7	10	18	57	71½
	9	38	18	58	64
20	0 M	18	18	58	60
	8	20	18	57	68½
	9	10	18	57	70
	11	5	19	1	75½
	0 A	40	19	6	76½
	4	2	19	4	74
	7	1	18	57	69½
	9	3	18	57	64
21	0 M	25	18	56	61
	8	0	18	56	71
	9	10	19	2	73
	10	6	19	4	73
	10	45	19	5	74
	11	16	19	6	75½
	0 A	2	19	5	77
	0	32	19	6	77½
	1	7	19	7	78½
	1	31	19	8	78½
	2	5	19	7	78
	2	32	19	6	79
	3	5	19	5	77
	3	35	19	4	76½
	4	21	19	1	75
22	0 M	31	19	1	61½
	8	0	18	56	64½
	8	50	19	0	65½
	10	10	19	3	70
	11	5	19	5	71
	1 A	25	19	8	74

June.

D.	H.	M.	°	′	Th.
22	9 A	12	19	2	67
23	8 M	0	18	56	65
	9	15	18	58	68½
	11	10	19	3	68
	1 A	3	19	8	67½
	1	40	19	7	67½
	3	16	19	5	67
	5	35	19	0	66
	9	14	19	1	62
24	0 M	32	19	1	58
	8	0	18	56	63
	9	20	18	57	65½
	11	6	19	3	69
	1 A	5	19	7	71
	1	48	19	8	70
	5	20	19	3	68
	9	0	19	1	61½
25	0 M	50	19	1	56
	8	0	18	56	62
	8	55	19	1	63
	11	20	19	11	68
	0 A	40	19	14	68½
	1	15	19	13	70
	2	56	19	9	70½
	5	0	19	1	68
	7	16	19	1	64
	9	30	19	2	63
26	0 M	1	19	2	61
	8	0	18	53	65
	8	45	18	55	65½
	10	57	19	5	69
	11	50	19	9	72
	0 A	31	19	11	72½
	1	10	19	11	72

	June.						June.				
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
26	7 A	0	19	1	68	30	9 A	0	19	0	58
27	0 M	18	19	2	62		9	45	19	0	58

	July.				

D.	H.	M.	°	′	Th.

	June.						July.				
	6	4	18	58	62	1	0 M	25	19	0	57½
	8	30	18	55	65		9	5	18	52	60½
	9	2	18	54	67		11	4	18	57	60
	10	20	18	57	69		0 A	12	19	0	61
	11	40	19	4	68½		1	0	19	3	61
	0 A	50	19	9	70		1	35	19	3	60½
	1	38	19	8	70		3	0	19	2	60
	3	10	19	8	68		5	5	19	1	58
	7	20	18	59	61		7	0	19	0	56½
	9	12	19	6	59	2	7 M	58	18	54	57
	11	40	18	51	57½		8	50	18	53	59
28	0 M	10	18	50	57		11	15	18	57	63
	8	14	18	56	61		1 A	15	19	1	66
	9	1	18	57	63		1	52	19	2	67
	11	1	19	1	67		3	5	19	3	65½
	0 A	35	19	7	68		5	4	19	1	63
	1	15	19	10	67½		9	15	19	0	59
	2	15	19	10	68½	3	0 M	8	18	49	57
29	0 M	14	19	0	57		8	20	18	45	60
	7	55	19	1	60		9	15	18	53	62
	8	58	18	59	63		11	0	19	1	67
	11	0	19	5	67		1 A	3	19	5	71
	0 A	35	19	9	69½		3	0	19	4	75
	1	15	19	11	67		7	30	18	59	67
	2	12	19	12	67½	4	2 M	0	18	56	56
	5	4	19	4	64¼		8	25	18	54	63
	7	10	19	0	61		9	10	18	57	64
30	0 M	20	19	0	56		11	0	19	0	66
	8	3	18	56	58						
	9	30	18	55	62						
	11	5	19	0	64						
	3 A	55	19	4	62½						

D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
			July.						July.		
4	1 A	25	19	4	69½	9	9 M	15	18	59	73
	2	20	19	6	71		11	20	19	1	76½
	3	0	19	6	70½		0 A	40	19	7	78
	5	5	18	52	68		1	20	19	6	78
	7	45	19	0	63		2	10	19	12	79
	9	10	19	0	59		6	50	19	0	73
5	1 M	3	19	2	54		9	20	18	49	68
	8	0	18	54	62		9	40	18	53	67
	10	57	19	1	68	10	8 M	0	18	53	72
	0 A	58	19	6	71		9	15	18	54	78½
	2	10	19	5	72		11	5	18	59	82½
	4	7	18	57	72		7 A	3	19	1	73
	11	30	18	59	62		9	25	19	0	68
6	8 M	20	18	56	68	11	8 M	12	18	54	68
	9	25	18	59	71½		11	20	19	2	72
	11	20	19	4	75½		1 A	0	19	7	72
	1 A	15	19	2	78		1	45	19	8	72½
	2	5	19	2	78½		3	20	19	7	71½
	3	20	19	3	78½		9	22	19	2	61
	5	15	19	0	77	12	0 M	15	19	3	58
	9	12	18	58	70		7	55	19	0	66
7	0 M	32	18	58	67		9	10	19	1	69
	8	17	18	50	73		0 A	20	19	11	70
	9	18	18	52	76		9	5	19	3	65
	11	20	18	57	79	13	8 M	20	18	53	68½
	1 A	30	19	2	81		11	12	19	2	73
	3	0	19	0	80		1 A	23	19	9	74½
8	0 M	5	18	57	64		2	0	19	10	76½
	9	10	18	54	77		9	10	18	58	66
	0 A	58	19	4	81	14	0 M	30	19	0	60½
	1	54	19	5	80½		8	25	18	55	67
	5	0	19	4	74		9	10	18	58	68½
	9	53	19	0	67		11	30	19	7	72
9	8 M	0	18	58	69		1 A	20	19	8	74

July.						July.					
D.	H.	M.	°	′	Th.	D	H.	M.	°	′	Th.
14	2 A	30	19	7	74		11 A	40	19	1	63
	5	0	19	4	73		8 M	0	18	57	67½
	9	5	19	2	67		9	18	19	0	70
15	0 M	15	18	57	61		11	15	19	3	74
	9	5	18	58	70		1 A	35	19	7	73
	11	12	19	4	73		3	30	19	4	72
	1 A	3	19	9	74½		7	0	19	1	68½
	1	45	19	8	75		11	50	19	3	61½
	5	0	19	3	71	21	8 M	5	18	57	66
	7	15	19	2	67		11	20	19	5	72½
16	0 M	30	19	2	62		1 A	20	19	8	73½
	7	53	18	57	68		2	15	19	9	74
	9	55	19	0	67	22	0 M	20	19	2	59½
	11	30	19	4	73		9	0	18	57	69
	1 A	4	19	7	75		10	58	19	2	74
	2	5	19	8	75½		5 A	0	19	3	75
	6	50	19	3	68	23	8 M	0	18	59	70
	9	15	19	5	65		9	5	19	0	72½
17	0 M	8	19	4	63½		11	5	19	4	77
	8	20	19	1	66		1 A	5	19	8	78
	11	18	19	8	71		1	53	19	9	78½
	4 A	10	19	9	72		3	15	19	4	78½
	7	0	19	1	66		5	16	19	0	78½
18	0 M	10	19	0	60		9	30	19	1	65
	8	0	18	58	64	24	0 M	10	19	1	61½
	9	7	18	59	66		7	46	18	57	66
	11	15	19	4	72		9	15	19	0	69½
	1 A	35	19	7	73		11	44	19	7	72
	2	15	19	6	73½		1 A	15	19	9	75
	9	35	19	0	65		3	0	19	7	74½
19	7 M	45	18	57	65½		6	12	19	2	71½
	9	25	19	2	71	25	0 M	2	18	57	61
	11	30	19	8	74½		8	20	18	54	68½
	5 A	2	19	2	75		9	35	18	57	70

July.

D.	H.	M.	°	′	Th.
25	11 M	10	19	4	74
	1 A	20	19	11	77½
	1	50	19	12	78
	3	7	19	10	77
	9	30	19	1	67
	11	50	19	2	62
26	8 M	0	18	58	66½
	9	17	19	1	71½
	11	43	19	8	77½
	1 A	17	19	11	78½
	2	7	19	11	79
	11	40	19	1	63
27	8 M	15	18	55	66
	9	12	18	57	66½
	11	5	19	4	72
	1 A	2	19	12	72
	1	30	19	12	72
	2	56	19	11	71
	5	10	19	6	69½
28	3 M	20	18	59	55
	8	30	19	1	66
	9	25	19	1	67
	11	3	19	6	70½
	1 A	2	19	12	73
	2	5	19	14	72
	3	40	19	12	74
	5	12	19	12	70½
	9	20	19	4	62
	11	57	18	57	59½
29	8 M	25	18	57	66
	9	20	18	57	69
	11	10	19	1	72
	1 A	35	19	11	73
	2	40	19	13	74½

July

D.	H.	M.	°	′	Th.
29	4 A	40	19	11	72½
	7	10	19	7	69
	10	0	19	5	66
30	0 M	30	19	2	65
	8	0	19	4	65½
	8	50	19	1	68
	9	25	19	0	67½
	11	24	19	6	73
	1 A	15	19	9	74
	2	5	19	12	74½
	3	16	19	12	73
	5	25	19	9	71½
	6	54	19	6	68
	9	5	19	4	64
	11	45	19	3	62
31	7 M	58	18	57	63
	9	15	19	0	67
	11	5	19	9	63
	1 A	12	19	15	62
	1	35	19	16	62
	3	2	19	11	61½
	4	45	19	8	60
	7	2	19	5	60½
	9	23	19	5	58

August.

D.	H.	M.	°	′	Th.
1	0 M	7	19	4	57
	8	30	18	58	62
	11	12	19	6	66
	2 A	7	19	11	68
2	0 M	10	19	1	57
	8	25	19	0	60

Auguſt.

August.						August.					
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
2	11 M	35	19	6	65	7	0 M	10	18	55	59
	1 A	26	19	11	68		7	40	18	58	60½
	9	38	19	4	56½		9	7	18	59	63
3	8 M	0	18	59	58½		11	20	19	4	66
	11	15	19	2	66		1 A	12	19	9	67½
	1 A	9	19	8	68		9	15	18	56	68
	2	10	19	9	68½	8	0 M	4	18	58	65
	5	20	19	0	67		8	15	18	58	68½
	7	15	18	59	63½		9	20	18	59	69½
	9	45	19	0	59		11	35	19	4	73
4	0 M	10	18	59	56		1 A	2	19	7	73½
	7	45	18	56	58½		2	55	19	2	77
	8	48	18	56	61		6	45	18	58	72
	11	2	19	3	61	9	0 M	32	18	57	63
	1 A	22	19	9	61		8	25	18	58	64
	2	40	19	6	61		11	12	19	6	75
	5	5	19	1	61		1 A	30	19	8	76
	7	6	19	1	61½		9	47	18	58	65
	9	30	19	1	62	10	8 M	12	18	57	70
5	0 M	12	19	2	61		9	15	18	57	74
	9	8	19	0	66½		11	0	19	2	79
	11	10	19	4	63		0 A	50	19	4	80½
	0 A	58	19	6	69		1	51	19	5	81
	4	37	19	3	63		3	32	19	2	79
	7	0	18	58	65		9	2	18	58	67
	9	2	18	59	63	11	0 M	30	18	57	63
6	7 M	32	18	55	58		8	15	18	53	68½
	9	11	18	56	62		9	13	18	56	73
	11	15	19	4	65½		11	2	19	0	77
	0 A	44	19	8	69½		1 A	17	19	4	81
	1	31	19	8	67		3	10	19	4	80½
	3	18	19	4	66½		6	15	18	58	77
	5	25	19	0	65½	12	0 M	12	18	59	67
	9	20	18	58	60½		8	25	18	58	62

D.	H.	M.	°	'	Th.
12	0	?	?	4	05
	0	?	9	8	65
	1	0	19	9	65
	2	30	19	7	67
	7	0	19	0	64
13	0 M	28	19	1	60
	7	55	18	58	62
	9	20	19	1	65
	11	21	19	9	70
	1 A	25	19	9	71
	3	10	19	7	71
	7	13	19	2	64
	9	25	19	2	59
14	0 M	30	19	2	55
	8	10	18	58	61
	9	20	18	59	62
	1 A	0	19	8	67
	7	2	19	2	64
15	0 M	38	19	1	61
	7	50	18	57	62½
	9	30	18	59	65
	11	15	19	3	69
	1 A	17	19	6	70
	2	0	19	5	67
	5	30	19	2	6 1/12
	9	10	19	3	63
16	0 M	2	19	2	60
	8	4	18	56	60½
	9	47	18	59	65
	11	10	19	4	67
	1 A	4	19	11	69½
	1	32	19	10	70
	3	0	19	10	71
	5	12	19	4	69

August.

D	H.	M.	°	'	Th.
16	9 A	40	18	51	63
	10	12	18	49	62¼
17	0 M	10	18	54	60
	8	30	18	56	64
	11	28	19	7	68½
	1 A	20	19	9	71
	4	0	19	6	69¼
	6	0	19	9	67
	7	2	18	58	65
	8	45	19	5	61
18	0 M	23	18	58	55
	8	15	18	58	58
	9	32	19	0	63
	10	58	19	5	67
	1 A	4	19	7	69
	3	20	19	4	68
	5	4	19	3	66¼
	6	52	18	59	65
19	0 M	40	18	59	59
	9	15	18	58	63¼
	10	58	19	3	67
	1 A	5	19	4	69
	1	52	19	4	68¼
	5	55	18	59	67
	7	15	19	0	65
20	0 M	42	19	1	60
	8	16	18	54	60½
	9	25	18	57	64
	11	7	19	4	65½
	0 A	40	19	7	67
	2	0	19	6	67
	5	10	19	0	65
	9	12	18	59	61
21	8 M	20	18	53	59½

An Aurora Bor.

August.

August.						August.					
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
21	9 M	22	18	56	62	25	3 A	2	19	10	71½
	11	3	19	8	66½		9	30	19	3	60
	0 A	40	19	11	62	26	0 M	10	19	4	58
	3	32	19	2	65½		9	5	19	1	65
	5	20	18	59	63		11	10	19	5	70
	6 ·	2	18	58	62		1 A	0	19	12	72
	9	12	18	59	59		2	15	19	14	72
22	0 M	15	19	1	54		5	7	19	5	68½
	8	3	18	55	57½		7	16	19	4	64
	11	10	19	7	62½		8	55	18	54	61
	0 A	54	19	9	63	27	9 M	15	19	1	63
	3	15	19	5	64		11	12	19	8	68
	6	53	18	59	60		0 A	40	19	13	69½
	9	20	19	0	56		1	30	19	14	70½
23	8 M	7	18	54	55		3	13	19	8	70½
	9	15	18	58	58		5	8	19	9	68½
	11	43	19	11	62		5	43	19	4	68
	1 A	0	19	14	66		7	15	19	4	64
	3	2	19	9	63		9	0	19	2	59½
	4	23	19	4	63½	28	0 M	5	19	7	56½
	5	54	19	3	62½		9	44	19	3	63
	9	15	19	4	58½		10	43	19	7	65
24	0 M	6	19	6	56		1 A	0	19	14	68
	8	20	19	3	59½		9	10	19	4	61
	10	8	19	9	63	29	10 M	25	19	6	65
	11	35	19	14	66½		1 A	30	19	13	67½
	1 A	9	19	24	67	30	0 M	25	19	4	51
	2	0	19	19	63		8	30	19	1	54
	4	55	19	8	62		1 A	15	19	15	62
	7	12	19	4	60		9	12	19	6	55
25	0 M	5	19	2	58	31	8 M	58	18	59	55½
	9	10	18	59	66		11	55	19	14	55
	11	20	19	9	70		1 A	14	19	16	55½
	1 A	15	19	12	72		5	15	19	5	57

August.

D.	H.	M.	°	′	Th.
31	8 A	32	19	5	52½

September.

D.	H.	M.	°	′	Th.
1	9 M	33	18	59	55
	0 A	59	19	12	59
	9	5	19	3	57½
2	10 M	0	19	0	59½
	1 A	4	19	9	63
	2	50	19	12	62
	5	35	19	8	59
	6	43	19	1	58
	9	44	19	3	60
3	9 M	35	19	1	64
	1 A	58	19	12	68
	3	15	19	8	69
4	0 M	20	19	3	58
	9	3	19	4	59
	11	15	19	7	63
	1 A	14	19	11	65
	5	34	18	57	61
	8	40	19	1	56
5	0 M	13	19	6	54
	8	42	19	7	55
	11	2	19	9	59
	1 A	15	19	12	59½
	2	5	19	13	60½
6	8 M	45	18	59	60½
	11	56	19	9	65½
	1 A	15	19	10	67
7	0 M	30	19	5	58
	9	10	19	3	61½
	1 A	5	19	16	66

September.

D.	H.	M.	°	′	Th.
7	3 A	2	19	10	65
8	1 M	46	19	4	57
	9	15	19	1	60
	11	6	19	6	64
	0 A	32	19	10	66
9	0 M	46	19	4	58
	9	42	19	5	66
	11	35	19	11	72
	0 A	56	19	10	73
	6	51	19	4	69
	9	7	19	4	68
10	0 M	40	18	37	66
	8	32	19	1	68½
	10	58	19	7	75
	0 A	56	19	18	77½
	1	15	19	20	78
	3	40	19	13	77½
	9	2	19	4	67
11	0 M	20	19	4	63½
	8	22	19	4	62
	11	4	19	11	63½
	1 A	20	19	14	64½
	2	50	19	16	64
	5	25	19	7	62½
12	0 M	23	18	54	55½
	8	30	19	8	59
	9	30	19	1	61½
	11	20	19	9	66
	1 A	25	19	13	67
	7	10	19	5	62
13	0 M	54	19	4	61
	9	20	19	2	67
	11	45	19	11	69
	1 A	30	19	12	70

September.					September.				
D.	H. M.	°	′	Th.	D.	H. M.	°	′	Th.
13	4 A 32	19	6	67	17	11 A 55	19	5	56
14	0 M 20	19	3	61	18	8 M 16	19	4	55
	8 15	19	1	58		11 25	19	15	61½
	9 25	19	3	60		6 A 0	19	5	57
	11 20	19	9	63		11 42	19	4	50½
	1 A 30	19	11	66	19	8 M 35	19	5	53
	3 40	19	10	63½		9 40	19	16	55
	5 32	19	7	61½		11 30	19	15	59
	11 55	19	2	54		1 A 32	19	19	60½
15	8 M 25	19	0	54½		3 2	19	12	60
	11 12	19	14	60		8 55	19	8	53
	1 A 30	19	13	61	20	0 M 20	19	2	48½
	8 55	18	53	54		8 40	19	1	50½
	11 58	18	53	53		11 20	19	9	58
16	9 M 30	19	9	58		1 A 40	19	1	60
	11 7	19	10	63		3 32	19	11	58½
	1 A 55	19	7	64½	21	0 M 2	19	2	48
	1 58	18	53	64½		8 45	19	1	56
	2 10	19	15	64½		11 10	19	9	60
	7 51	20	14	56		1 A 15	19	13	62
	7 52½	19	30	56		5 28	19	6	58½
	7 54½	19	14	56		9 16	19	1	54½
	7 57½	17	56	56		11 57	19	3	53
	7 59	17	44	56	22	8 M 28	19	0	56½
	8 5	18	34	56		11 4	19	5	63
	8 11	18	43	56		1 A 30	19	13	63½
	8 18	19	22	55½		3 18	19	12	63
	8 29	19	5	55		6 17	19	10	57
	9 25	18	55	53		9 5	19	4	52
17	8 M 15	19	1	56	23	9 M 55	19	5	59
	11 6	19	9	63		0 A 50	19	13	64
	1 A 17	19	13	64		7 22	19	6	67
	3 10	19	9	65	24	8 M 17	18	58	53
	7 0	19	4	61		11 30	19	9	62

A strong Aurora Borealis

	September.				
D.	H.	M.	°	′	Th.
24	1 A	35	19	16	65
	3	14	19	13	63
	8	54	18	53	56½
	9	18	18	54	56
	11	52	19	18	53½
25	8 M	25	18	59	53
	10	2	19	3	54½
	11	15	19	8	56½
	1 A	18	19	13	58
	5	16	19	8	58½
	9	24	18	58	57
26	0 M	16	19	1	55
	8	46	19	0	57
	11	30	19	11	57
	1 A	7	19	12	59
	2	15	19	14	60
	3	12	19	15	58
	5	40	19	11	55
	11	45	19	14	49
27	8 M	43	19	1	53
	9	30	19	4	55
	1 A	0	19	12	59
	1	52	19	12	59½
	9	16	19	5	53
28	8 M	17	19	18	60
	11	40	19	14	62
	1 A	27	19	15	63½
	2	50	19	10	63
	5	20	19	5	59
	7	0	19	4	58
29	0 M	5	19	5	55
	9	15	19	1	59
	11	44	19	11	62
	1 A	35	19	11	63

(left margin, against rows 28–29: An Aur. Bor.)

	September.				
D.	H.	M.	°	′	Th.
29	9 A	7	19	4	51
30	9 M	46	19	1	55
	0 A	5	19	14	59
	1	40	19	12	61
	11	44	19	4	48

	October.				
D.	H.	M.	°	′	Th.
1	8 M	6	18	58	47
	9	7	19	1	48
	2 A	0	19	14	62
	8	52	19	2	52
	11	50	19	3	49½
2	8 M	20	19	14	54
	11	10	19	10	59½
	1 A	12	19	14	60
	8	10	19	4	52
	9	21	18	49	49½
3	0 M	12	18	44	48
	8	41	19	9	48
	10	2	19	8	50½
	11	44	19	14	53
	1 A	35	19	15	54
	4	8	19	9	52½
4	8 M	35	19	0	54
	11	14	19	9	58½
	1 A	25	19	12	59
	9	11	19	3	53½
5	8 M	25	18	58	50½
	9	46	19	0	56
	11	15	19	6	59½
	1 A	12	19	10	62
	2	30	19	8	62

(left margin, against rows 2–3: Aur. Bor.)

	October.						October.		
D.	H. M	°	'	Ta.	D	H. M	°	'	Ta.
5	9 A 18	19	6	58½	12	11 M 22	19	7	59
	11 16	19	4	58		1 A 20	19	12	60
6	9 M 0	19	1	60		3 2	19	8	60
	11 32	19	8	63		5 38	19	5	58
	1 A 33	19	12	65		9 7	19	4	54
	3 7	19	9	65½		11 46	19	3	51
	9 15	19	4	60	13	8 M 32	18	57	52
7	0 M 22	19	5	56		11 30	19	7	57
	10 1	19	1	63		1 A 28	19	14	58
	1 A 23	19	9	67		9 44	19	3	50
	9 10	19	4	57	14	10 M 7	19	3	56
8	8 M 12	18	55	52		11 45	19	9	55
	10 58	19	4	63		0 A 57	19	10	54
	1 A 30	19	24	70		5 28	19	4	53½
	3 2	19	17	69½		9 34	19	4	51
	4 24	19	23	67½	15	9 M 0	18	59	49
9	0 M 28	18	26	56		10 52	19	7	53½
	8 26	19	23	54		1 A 13	19	10	57
	11 15	19	14	67½		3 15	19	8	57
	4 A 21	19	7	68		9 18	19	4	56
	9 40	18	54	60	16	0 M 32	19	5	55
10	8 M 30	19	8	60½		8 31	18	59	56
	9 51	19	7	64		11 20	19	10	59
	11 5	19	11	65		1 A 26	19	11	61
	1 A 12	19	12	66		5 24	19	4	58
	3 55	19	7	65		9 16	19	5	59
	7 20	18	57	62	17	9 M 8	19	2	61
11	8 M 7	19	3	58		11 16	19	11	61
	11 13	19	7	60		1 A 28	19	10	59
	1 A 32	19	10	61		3 27	19	6	57
	3 15	19	9	59½		6 24	19	5	53
	5 30	19	5	58½	18	10 M 13	19	4	50
12	0 M 2	18	57	56		1 A 0	19	10	52
	8 31	18	58	58½		8 3	19	5	47½

Aur. Bor.

October.

October.					October.			
D.	H. M.	° '	Th.	D	H. M.	° '	Th.
19	8 M 41	19 1	42½	26	11 A 55	19 9	51½
	1 A 8	19 14	50	27	8 M 32	19 1	50
	3 12	19 13	49		1 A 17	19 14	47
	9 10	19 5	43		3 46	19 13	45½
20	9 M 32	19 2	39		9 2	19 7	41
	11 9	19 9	41½	28	9 M 46	19 4	42
	0 A 50	19 12	43		11 12	19 10	49½
	9 24	19 5	39½		1 A 3	19 12	52
21	3 A 7	19 9	53		5 10	19 9	49
	10 54	19 5	52		9 12	19 7	48
22	10 M 9	19 6	57	29	8 M 55	19 2	49
	0 A 44	19 11	59		11 0	19 8	52
	6 26	19 7	57		1 A 22	19 14	55
23	0 M 14	19 4	51½		3 35	19 12	54
	8 31	19 2	47		8 46	19 2	52
	11 32	19 9	55	30	8 M 40	19 6	50½
	1 A 4	19 10	56		11 8	19 15	52½
	5 8	19 8	52		1 A 6	19 17	53
	7 4	19 7	50		4 18	19 10	50
	11 41	19 7	45		9 32	19 4	44½
24	8 M 36	19 2	43	31	8 M 36	19 9	42
	1 A 8	19 14	53		9 22	19 7	44
	3 10	19 12	51		1 A 22	19 18	50
	11 55	19 8	43		3 40	19 16	49
25	8 M 46	19 2	49		5 45	19 13	46
	11 9	19 9	53		9 32	18 54	42
	1 A 28	19 13	55				
	2 57	19 11	54½	November.			
	7 52	19 8	50				
26	8 M 32	19 2	45	D.	H. M.	° '	Th.
	11 20	19 10	48½	1	0 M 21	18 39	39
	1 A 13	19 11	50		9 58	19 14	43
	3 0	19 9	51½		1 A 38	19 11	46½
	8 56	19 7	50½		11 52	19 7	38

A faint Aur Bor.

D.	H. M.	°	′	Th.	D.	H. M.	°	′	Th.
	November.					November.			
2	8 M 54	19	4	44	9	9 M 48	19	1¼	45
	1 A 26	19	10	50		1 A 37	19	18	50
	3 50	19	8	47½		9 35	19	3	52
	9 28	19	7	39½	10	8 M 35	19	7	46
3	8 M 40	19	4	37½		1 A 6	19	15	48½
	10 1	19	6	43		8 46	19	3	41
	11 24	19	10	47	11	10 M 7	19	7	42
	1 A 36	19	11	51		11 21	19	11	43½
	9 12	19	5	44		1 A 4	19	14	45
4	0 M 46	19	6	40		2 25	19	14	45
	10 2	19	6	48		7 28	19	10	40
	11 10	19	10	51	12	8 M 41	19	8	37½
	1 A 17	19	13	56		11 16	19	1¼	42
	5 17	19	9	55		1 A 6	19	15	44
5	0 M 25	19	6	55		3 20	19	12	42½
	9 36	19	3	57½		6 49	19	9	40
	1 A 28	19	17	58	13	8 M 36	19	8	40½
	6 54	19	4	54		11 29	19	15	43½
6	8 M 37	19	4	47½		1 A 7	19	16	46
	11 4	19	10	49		3 57	19	13	43
	1 A 7	19	15	51		9 58	19	9	38
	3 1	19	16	49	14	9 M 2	19	10	36
	5 16	19	12	45		1 A 48	19	15	43½
	9 17	19	10	44		3 4	19	14	42
7	8 M 14	19	9	46		7 46	19	9	37
	11 5	19	11	49	15	8 M 37	19	8	36
	1 A 26	19	14	52		10 55	19	12	41
	3 20	19	14	52		1 A 33	19	14	43
	7 17	19	10	47		4 36	19	10	40
8	0 M 46	19	11	42		11 30	19	9	33½
	8 52	19	11	42	16	8 M 47	19	5	31
	11 6	19	15	46½		11 47	19	14	38
	1 A 22	19	18	47		1 A 34	19	16	41½
9	0 M 31	19	12	44		3 36	19	14	40

November.						November.					
D.	H.	M.	°	′	Th.	D.	H.	M.	°	′	Th.
16	5 A	45	19	9	37½	23	8 A	52	19	6	31
17	9 M	16	19	7	32	24	0 M	14	19	3	31
	11	9	19	13	37		9	4	19	14	33½
	1 A	42	19	14	40		11	0	19	18	37
	3	26	19	13	38½		1 A	16	19	20	38
	9	5	19	7	33		4	22	19	9	39
19	9 M	7	19	7	32		7	23	19	6	39½
	11	46	19	15	38		9	57	19	5	39½
	1 A	15	19	15	38	25	10 M	2	19	7	42
	2	57	19	14	38		11	40	19	10	46
	6	3	19	10	35½		1 A	5	19	12	48
	9	38	19	7	34		4	22	19	9	49
20	0 M	13	19	9	35	26	8 M	58	19	8	53
	9	4	19	6	35½		10	56	19	13	54
	10	14	19	8	37		1 A	23	19	16	54
	11	35	19	10	38		3	2	19	14	53½
	2 A	53	19	14	36		7	46	18	57	49
	5	2	19	12	35		9	25	18	44	48
21	0 M	7	19	9	34½	27	8 M	42	19	2	42½
	9	5	19	5	37		11	12	19	26	46½
	11	15	19	10	37		1 A	0	19	19	48
	1 A	12	19	14	37		3	2	19	14	48
	3	11	19	14	36½		5	33	19	14	47
	8	20	19	8	35½		9	45	18	44	49
22	0 M	10	19	6	34	28	8 M	32	19	5	53
	9	4	19	8	29		0 A	7	19	13	53
	11	28	19	12	34		1	26	19	14	53
	1 A	7	19	13	37		2	52	19	11	51½
	3	12	19	13	36		6	0	19	7	47
	9	36	19	9	30		9	10	19	4	43
23	9 M	5	19	11	24	29	9 M	14	19	7	38½
	11	12	19	16	26		10	46	19	9	41
	1 A	30	19	18	33		1 A	20	19	10	44
	3	37	19	15	33		5	16	19	6	39

Faint Aur. Bor.

November.

D.	H.	M.	°	'	Th.
30	0 M	25	19	4	33
	11	16	19	10	36
	8 A	53	19	4	32½

December.

D.	H.	M.	°	'	Th.
1	9 M	16	19	8	31½
	11	17	19	13	34½
	1 A	30	19	13	37½
	2	50	19	10	38½
	8	52	18	59	41½
	9	30	18	44	41½
2	1 A	12	19	14	43
	7	44	19	5	47
3	11 M	9	19	12	39½
	1 A	58	19	15	43
	3	51	19	8	41
	9	30	19	4	39
4	9 M	5	19	6	36
	11	1	19	9	36
	1 A	9	19	10	36½
	3	2	19	8	36
5	8 M	53	19	6	34
	10	3	19	9	36
	2 A	26	19	10	41
	10	1	19	2	35
6	0 M	15	19	3	37
	9	5	19	4	39
	11	7	19	9	40
	0 A	45	19	9	39
	8	2	19	2	33½
7	8 M	58	19	3	34½
	10	22	19	5	35

December.

D.	H.	M.	°	'	Th.
7	1 A	35	19	8	37
	4	54	19	6	37
	9	32	19	4	36
8	0 M	25	19	5	35
	9	17	19	4	37½
	11	5	19	7	40
	1 A	4	19	11	42
	3	0	19	11	41
	5	37	19	9	37
	9	45	19	4	40
9	0 M	16	19	5	39
	10	37	19	8	38
	1 A	21	19	10	41
	3	40	19	9	40
	11	48	19	7	39
10	8 M	20	19	7	36
	10	24	19	9	38
	1 A	25	19	10	41
	3	48	19	9	40
	9	16	19	3	38½
11	0 M	15	19	4	37½
	9	2	19	5	35
	11	10	19	9	36½
	0 A	47	19	10	37½
	3	28	19	12	37
	9	40	19	6	34
12	9 M	14	19	7	30½
	11	3	19	9	34
	1 A	12	19	13	34½
	3	30	19	8	33
	9	5	19	6	30
13	9 M	2	19	7	28½
	10	10	19	12	30
	2 A	1	19	14	31

December.					
D.	H.	M.	°	'	Th.
13	8 A	58	19	4	30½
14	9 M	5	19	7	31
	11	7	19	9	33
	0 A	55	19	14	33
	3	20	19	12	32
	6	17	19	1	31
	9	8	19	5	30
15	9 M	25	19	7	31
	11	24	19	13	35
	1 A	30	19	17	36
	3	20	19	8	33½
16	0 M	15	19	7	29
	10	22	19	8	33
	1 A	16	19	13	36
	5	37	19	13	31½
	9	15	19	7	33½
17	9 M	0	19	6	26½
	11	12	19	10	32½
	1 A	40	19	11	34½
	3	8	19	12	33½
	9	25	19	8	26½
	11	56	19	7	25
18	9 M	7	19	7	25
	11	10	19	11	31
	1 A	12	19	14	33
	5	23	19	12	32½
	9	10	19	8	32
19	9 M	5	19	7	32½
	11	30	19	13	33½
	1 A	25	19	16	34
	5	37	19	9	34
	9	10	19	8	32
20	0 M	5	19	8	32½
	9	7	19	8	36

December.					
D.	H.	M.	°	'	Th.
20	10 M	32	19	9	37
	1 A	58	19	12	40
	4	10	19	11	41
21	9 M	5	19	8	37
	11	26	19	9	39
	1 A	0	19	11	39
	4	0	19	11	40
	6	16	19	10	40
	9	15	19	9	39
22	10 M	5	19	10	38
	11	56	19	12	39½
	1 A	49	19	16	40
	5	6	19	10	39
	8	50	19	9	38
23	0 M	48	19	8	37
	10	30	19	16	38
	5 A	2	19	9	39½
	5	10	19	4	39½
	5	17	19	14	39
	5	22	19	14	39
	6	30	19	1	39½
	7	53	19	2	37
24	0 M	40	19	6	37
	10	25	19	10	36
	1 A	40	19	11	36
	3	25	19	9	36
	6	41	19	6	36
	9	18	19	5	36
25	9 M	34	19	6	40
	11	30	19	9	42
	2 A	0	19	10	43½
	3	25	19	11	43
	5	56	19	10	43
	8	30	19	6	43

An Aurora Bor.

December.

December.				December.			
D.	H. M	° '	Th.	D.	H. M	° '	Th.
26	9 M 40	19 6	42½	29	3 A 15	19 14	45
	11 0	19 7	46		5 42	19 14	44½
	1 A 30	19 13	47		9 5	19 5	45
	3 0	19 12	46½	30	0 M 12	19 8	46
	7 22	19 10	42½		10 25	19 10	47
	9 4	19 8	42½		11 35	19 11	47
27	0 M 2	19 7	43		1 A 3	19 12	47
	10 24	19 17	38		2 0	19 13	46½
	1 A 28	19 18	42		5 43	19 11	46½
	3 2	19 13	41		9 32	19 9	47
	4 47	19 9	39½	31	0 M 33	19 9	46
28	11 M 23	19 14	45½		9 18	19 8	43
	1 A 15	19 18	47		11 2	19 9	45½
	6 30	19 9	46		1 A 14	19 14	47
	9 11	19 8	44		2 57	19 13	46
29	10 M 0	19 17	46		5 30	19 10	44
	0 A 47	19 14	46		9 2	19 8	42½
	1 43	19 17	45½		11 57	19 8	42

The mean diurnal Variation for each Month in the Year 1759.	'	"
January - - -	7	8
February - - -	8	58
March - - -	11	17
April - - -	12	26
May - - -	13	0
June - - -	13	21
July - - -	13	14
August - - -	12	19
September - -	11	43
October - - -	10	36
November - - -	8	9
December - - -	6	58

XXXIX. *A Letter to the Right Honourable* Hugh *Earl of* Marchmont, *F. R. S. concerning the Sections of a Solid, hitherto not considered by Geometers; from* William Brakenridge, *D. D. Rector of* St. Michael Baſſiſhaw London, *and F. R. S.*

My Lord,

Read Dec. 20, 1759.   **Y**OUR knowlege in Geometry, and the other Sciences that depend upon it, makes me preſume to lay before you the following ſpeculations. Your benevolence to all philoſophical Inquiries encourages me, and the perſonal regard I have for your Lordſhip induces me to do myſelf this honour; and tho' what I offer at preſent may be of no great conſequence, I am perſuaded, that every little acceſſion to our knowlege will give you ſome pleaſure, as you very well know, that all our improvements in ſcience are ſlow, and from ſmall beginnings. You have here a new method of conſidering ſome geometrical curves, from the ſections of a ſolid, hitheito not taken notice of, and by which, in particular, you will ſee, that the two infinite curve lines from the ſection of the cone, are alſo the ſections of this; which may be of ſome uſe, as it ſeems to extend our views of their nature and properties. The deſcription of it is very eaſy and obvious, and it has ſomething remarkable in its form, that tho', in the moſt ſimple caſe, it is generated and bounded by right lines, the ſurface is incurvated. The ſolid is thus deſcribed.

Let

Fig. 1.

Fig. 2.

Fig. 4.

Fig. 8.

Fig. 9.

Fig. 3.

Fig. 6.

Fig. 5.

Fig. 7.

Fig. 10.

Fig. 11.

J. Mynde fc.
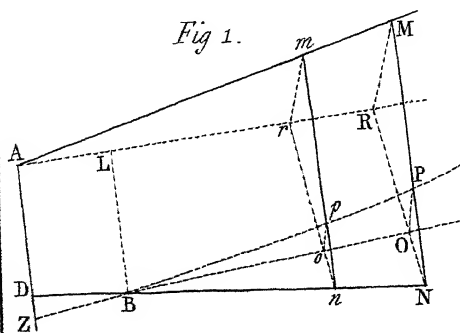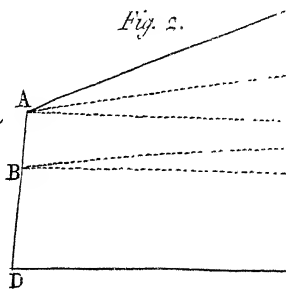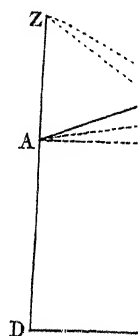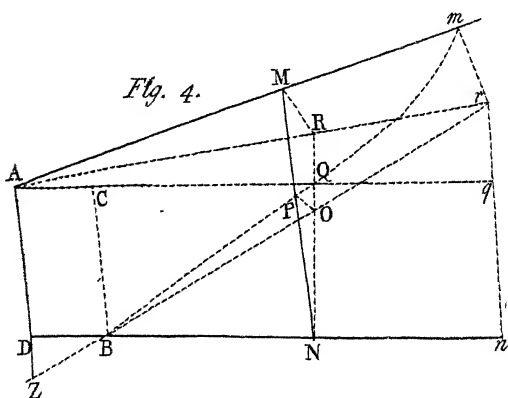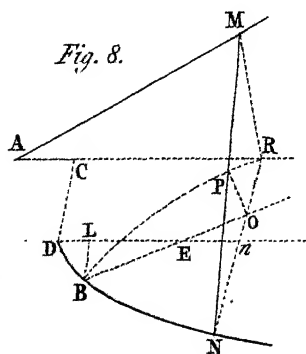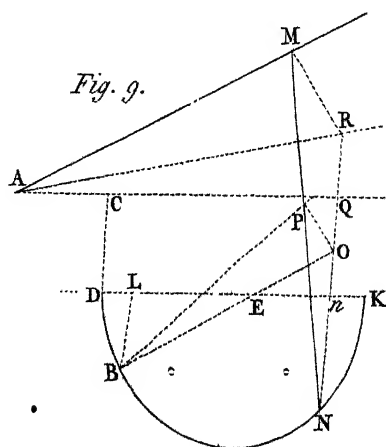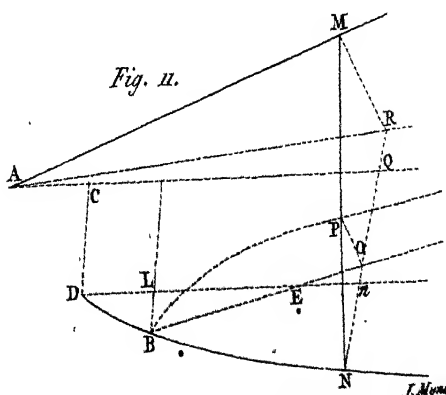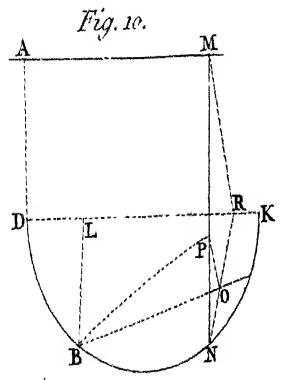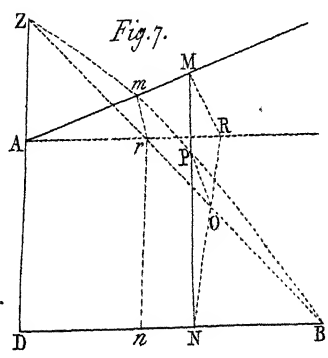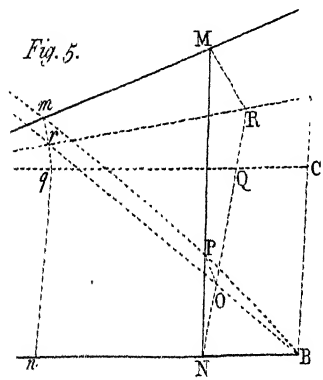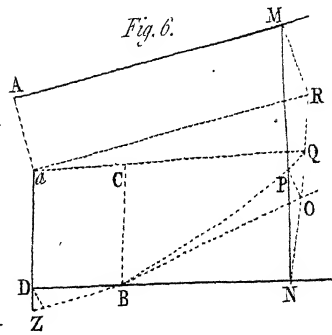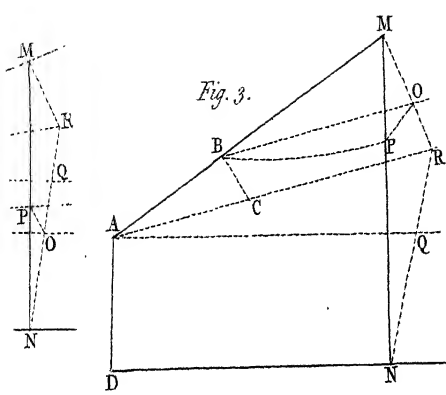
Let D N be a right line drawn on a plane, and a point given A at any diſtance from the line, from which having raiſed, above the plane, the right line A M in any given angle, and drawing from the ſame point A the line A D, meeting the line D N on the plane in D, make another plane M N R to move parallel to itſelf, and to that line A D in a given angle to the firſt plane; and then, ſuppoſe the interſections of it M, N, *m*, *n*, &c. with the lines A M and D N, to be continually joined with right lines M N, *m n*, &c. and there will be generated an incurvated ſurface by them, and bounded by the lines A M, M N, N D, A D. *Plate* X. *Fig.* 1.

The ſame ſurface will be deſcribed, if a line M N be ſuppoſed to move in ſuch manner, along the lines A M and D N, that the parts A M and D N be continually in a given ratio. For let a plane be drawn thro' the raiſed line A M, perpendicular to the given plane A D N, and their common interſection be the line A R; then having joined the points A and D, from the point N, while the line M N is moving, let there be continually drawn a line N R parallel to D A, meeting the line of interſection A R of the planes in R, and the proportion of D N to A R will always be given. But as the ratio of D N to A M is given, the ratio of A M to A R will alſo be given; and the line R M being drawn, the angle A R M will be given; and therefore the plane of the triangle M R N will move parallel to itſelf, and to the line A D, making the given angle M R A with the plane A D N, and the line M N will generate the ſame ſurface as before.

It

It is evident this incurvated surface may be infinitely extended on all sides beyond the lines A M, M N, N D, A D, and as well below the given plane A D N as above it; and therefore the various sections of it, if continued, will be infinite lines.

The line A M, raised above the given plane A D N, may be called the vertical Directrix.

And if thro' this directrix A M there be drawn a plane A M R in any given angle to the plane A D N, interfecting the moving plane N M R in the line M R, and meeting the given plane A D N in R; then the points A, R, and R, N, being joined, there will be a trapezium A R N D, formed, that may be named the Base, and in which the line D N may be called the Directrix of the Base.

And the plane A M R paffing thro' the vertical directrix and the moving plane M N R, together with the base A R N D, and the curve furface, will make a folid A M R N D A, fomething in the form of a wedge.

In this folid there may be made five fections in a given angle with the bafe, or parallel to it; viz, one parallel to the moved plane; one parallel to the directrix of the bafe; another parallel to the vertical directrix; a fourth parallel to the bafe; and a fifth interfecting both the directrices. And in all these cafes, when the directrices are right lines, the fections will be either the conic hyperbola, or the parabola, or right lines.

ift, If the folid A M R N D A be cut by a plane parallel to the moved or generating plane N M R, or the line A D, the fection will be a right line. This is evident from the defcription, *Fig.* i.

2.

2. If the section be made by a plane B P O parallel to the directrix of the base D N, and in any given angle to the base, it will be an hyperbola, *Fig* 2.

Let the section P B O meet the base in the line B O, which will be parallel to D N, and make the moving plane N M R intersect the base in N R, the vertical directrix in M, and the section in P O; by which D B=N O. From the point M draw M R parallel to P O, and imagine a plane to pass through the directrix A M and the line M R, meeting the base in A R; which will be given in position. Then thro' the point A draw A Q, parallel to D N, intersecting N R in Q, and making N Q=A D; and the two triangles A R Q, A M R, will have all their angles given, and the proportion of their sides. And therefore the ratio of A Q to Q R, and of A Q to M R, will be given. Make A D=a, B D=b, A Q : Q R :: $a:q$, and A Q : M R :: $a:m$, the abscisse B O=x, and the ordinate P O =y; from which Q R = $\frac{qx}{a}$,

N R=a+$\frac{qx}{a}$, M R=$\frac{mx}{a}$. And then from the similar triangles N O P, N M R, the analogy N O : O P :: N R : M R will give the equation $yxq + ya^2 = xbm$, and the curve B P is an hyperbola.

3. If the section P B be made by a plane parallel to the vertical directrix A M, it will be an hyperbola. *Fig.* 1.

Let the moving plane M R N, parallel to D A, intersect the base in R N, and the vertical directrix in M, and make the plane of the section B P O cut the moving plane N M R in P O, and the base in the line B O, meeting D A in Z, and the base directrix D N in B; from the point M draw the line

M R parallel to the plane of the section, and meeting the base in R ; and the lines M R and P O will be parallel. Then thro' the vertical A M, and the line M R, make a plane to pass, which will be parallel to the section, intersecting the base in A R, and the lines A R and B O will also be parallel. Draw B L parallel to D A, or N R, passing A R in L, and then B O = L R, B L = R O = A Z, and A L = B Z. And the line A R being given in position, the lines B L and A L will be given. The angles also M A R and N B O being given, the ratio of A R to R M, and of B O to O N, will be given. Suppose A D z = B L = a, A L = c the abscisse B O = x, the ordinate P O = y, and A R : R M : : a : m, and B O : O N : : a : n, from which we have O N = $\frac{nx}{a}$, R M = $\frac{mc + mx}{a}$;

N R = $a + \frac{nx}{a}$. And then, in the triangles on the moving plane, which are similar, M R N, P O N, the analogy N O : P O : : N R : R M will produce the equation $y a^3 + n x a y = x^2 n m + x n m c$; which shews the curve B P to be an hyperbola; and the figure is convex to the base.

4. The section being made parallel to the base, it will be the same curve. *Fig.* 3.

Having thro' the vertical directrix made a plane A M R to pass perpendicular to the base, let the section B P O, and the base parallel to it, meet that plane in the lines B O and A R, and also the moving plane N M R in the lines P O and N R. From the point A draw A Q parallel to the directrix D N of the base, and A D parallel to N R, and from B the line B C parallel to O R, meeting A R in C ; and then

then the lines A D, A C, and B C, will be given, and the angles R A Q, M A R, being alfo given, the proportion of A R to Q R, and of B O to M O, will be known. Make A D = a, A C = c, the abfciffe B O = x, the ordinate P O = y, and A R : R Q :: $a : q$; from which we have M O : M R :: $x : x$ + c, Q R = $\frac{c\,q + q\,x}{a}$. And becaufe, in the fimilar triangles M P O, and M N R, in the moving plane, we have M O : P O :: M R : R N, there will refult the equation $y\,x\,a + y\,c\,a = x^2 q + x\,c\,q + x\,a^2$; which denotes the curve B P to be an hyperbola.

5. If the fection is made fo as to meet the two directrices, the curve will be alfo an hyperbola. *Fig.* 4.

Let the fection B P O meet the directrices in B, m, and interfect the plane of the bafe in the line B O; and make the moving plane N M R to be cut by the fection in P O, and to meet the vertical directrix in M. Then from the point M draw M R parallel to P O; and thro' the lines A M, M R, imagine, as before, a plane to pafs, interfecting the bafe in the line A R r, and meeting the line B O in r, and the fection in $m r$. And from A draw A D parallel to N R, and A Q to D N; and thro' r make $n r$ parallel to N R, and to meet A Q in q, and D N in n. Draw alfo from the point B the line B C parallel to A D, and meeting A Q in C. The lines then A q, A r, $m r$, $r q$, B r, B n, $r n$, will be given. Make therefore AD = a = BC, AQ : QR :: $a : q$, and AQ : RM :: $a : m$, B r : B n :: $a : b$, B r : $r n$ :: $a : n$, A C = D B = c; the abfciffe B O = x; the ordinate O P = y. From which we have BN = $\frac{b\,x}{a}$, ON = $\frac{n\,x}{a}$, A Q = c +

$b\,x$

$\frac{b x}{a}$, $RQ = \frac{q c a + q b x}{a^2}$, $MR = \frac{m c a + m b x}{a^2}$. And then, in the fimilar triangles N P O, M R N, having $NO : PO :: NR : MR$, the equation will be $y a^4 + y c a^2 q + y x b a q = n m c a x + n m b x^2$. And the curve is an hyperbola; and in the cafe of this *Fig.* 4. it will be convex to the plane of the bafe. But when B N is negative in the cafe of *Fig.* 5. the equation, retaining the fame fymbols, will be $y a^4 + y q c a^2 - y x b q a = m n c a x - n m b x^2$; and the hyperbolic curve will be concave towards the bafe.

6. If the vertical directrix A M is made parallel to the plan of the bafe, but the plane paffing thro' it not parallel to the other directrix, then the fection, meeting the two directrices, will alfo be the fame curve. *Fig.* 6.

For in this cafe the line M R is a conftant quantity; and therefore, if the common fection *a* R of the plane thro' the vertical A M, with the bafe, meet *a* D parallel to N R in *a*, other being as before, making M R = *m*, and D *a* = *a*; from the analogy $NO : PO :: NR : MR$ in the triangles N O P, M R N, we fhall have $m n a x = y a^3 + y a c q + y x q b$; by which the curve is known to be an hyperbola.

And in all thofe fections, where the common interfection of the plane, paffing thro' the vertical with the bafe, is not parallel to the other directrix, the curve is an hyperbola.

7. But now, if we fuppofe the common interfection A R of the plane paffing thro' the vertical, with the bafe, to be parallel to the directrix D N of the bafe, and both directrices to be cut by the fection, the curve will be a parabola. *Fig.* 7.

For

For, in this cafe, the two lines A R and A Q co-incide, and A R is parallel to the directrix D N of the bafe; and therefore, ufing the fame fymbols as above, the equation, from *Fig.* 4. will be reduced to $ya^4 = nmcax + nmbx^2$; and from *Fig.* 5. it will come to $ya^4 = nmcax - nmbx^2$; both which fhew the curve to be a parabola.

It may alfo be demonftrated in this manner. It is evident, that $PO : mr :: PO \times MR : mr \times MR$, that is, the ratio of P O to mr is equal to that compounded of $PO : MR$, and of $MR : mr$. But from the fimilar triangles M R N, P O N, we have $PO : MR :: NO : NR = nr$, and from the triangles B O N, B r n, we have $BO : Br :: NO : nr$; therefore $PO : MR :: BO : Br$. In like manner, from the equi-angular triangles M R A, mr A, there will be $MR : mr :: RA : rA$, and from the triangles R O r, Z A R, it is $RA : rA :: Oz : rz$; therefore $MR : mr : oz : rz$. If then in the ratio of $PO \times RM$ to $mr \times MR$, we fubftitute the ratio of $BO : Br$, and of $Oz : rz$, which are equal to $PO : MR$, and $MR : mr$, we fhall have $PO : mr :: BO \times oz : Br \times rz$; which is a known property of the parabola.

And thus I have endeavoured to extend, a little, the theory of the conic fections. I have here fhewn how two of them may be had from the fections of this folid; and in the year 1733, I publifhed, in my *Exercitatio Geometrica*, a method of defcribing all of them on a plane, by the moving of three right lines about three given points, two of the interfections being drawn along two other lines given in pofition; the only hint of which I had from a locus of an ellipfis, in the conftruction of a clock, by the celebrated M. De

la Hire, in the French *Memoires* for the year 1717, intituled, *Conſtruction d'une Horologe qui marque le vrai tems avec le moyen*.

But now, having briefly conſidered the various ſections of this ſolid, the directrix D N of the baſe being a right line, let us next ſee what they are when it is a curve line of any order. And becauſe there are an infinite number of caſes, it will be ſufficient to mention a few of them, when that directrix is a conic ſection or circle, and then to give a general propoſition when it is any higher geometrical curve.

8. If the directrix D N of the baſe be a parabola, having a diameter parallel to the interſection A R of the plane, paſſing through the vertical A, and both directrices be cut by the ſection, the curve B P will be a line of the fourth order. *Fig.* 8.

- Let the diameter be D L n, the ordinate N n, and the equation of the parabola $u^2 = z p$, and make the moving plan M N R to paſs through the ordinate N n, and the ſection to interſect the baſe in B O, and the diameter in E. From the points B and D draw B L and D C, parallel to N O, and meeting the lines A R and D n in C and L. Then ſuppoſe A R : R M : : a : m, and B O : L n : : a : n, A C $= c$, D C $= d$, B L $= b$, D L $= l$, B E $= e$, N $n = u$, D $n = z$, the abſciſſe B O $= x$, the ordinate P O $= y$; and we have

$$L n = z - l = \frac{n x}{a}, \quad n o = \frac{b \times \overline{x - e}}{e}, \quad M R = \frac{m c + m z}{a}$$

$$= \frac{m c a + m a l + n x m}{a^2} \text{ and } u^2 = \frac{a p l + p x n}{a}. \text{ But from}$$

the ſimilar triangles N P O and N M R we have this proportion N O : P O : : N R : M R, which gives the

equation $\dfrac{\overline{b x - b e} \times \overline{m c a + m a l + n m x - d y a^2 e}}{y a^2 e - m c a e - m a l e - n m x e} = u$

$= \sqrt{\frac{apl + pxn}{a}}$; and this being reduced, fhews, that the fection P B is a line of the fourth order. But if the fection be made parallel to the vertical, the curve will be a line of the third order.

9. If the directrix D N be a circle, and other things being as before, the fection will be a line of the fourth order. *Fig. 9.*

Make the center of the circle to be in the line D L n parallel to A Q, the ordinate to be N $n = u$, the abfciffe D $n = z$, the radius $= r$, and the equation $u^2 = 2rz - z^2$. And let the plane paffing through the vertical, cut the bafe in A R, and the fection meet the diameter D K in E. Then the fame things being fuppofed, and the fymbols retained as before, we fhall have L $n = z - l = \frac{xn}{a}$, $z = \frac{al + xn}{a}$,

$n O = \frac{bx - eb}{e}$, Q R $= \frac{qac + qal + qnx}{a^2}$, M R $=$

$\frac{mac + mal + mnx}{a^2}$, $\sqrt{\frac{2rla^2 + 2rxna - x^2n^2 - 2alnx - a^2l^2}{a^2}}$

$= u$. And the analogy N O : P O :: N R : M R will give this equation, $\frac{mac + mal + mnx}{a^2} \times \frac{be - bx}{e}$

$+ \frac{ya^2d + yqac + yqal + yqnx}{a^2} = \frac{mac + mal + mnx - a^2y}{a^2}$

$\times \sqrt{\frac{2rla^2 + 2rxna - x^2n^2 - 2alnx - a^2l^2}{a^2}}$; from which it appears, that the curve B P is a line of the fourth order.

And in general it may be feen, that if the directrix of the bafe be a conic fection, except in the cafe above, the fection of the folid will be a line of the fourth order.

10. If

10. If the vertical be parallel to the bafe, and the plane paffing through it perpendicular, the directrix of the bafe being a circle, having its center in the interfection A R of the two planes; then the folid will be the cono-cuneus of the learned Dr. Wallis, and the curve fections of it will be alfo, fome cafes excepted, lines of the fourth order. *Fig.* 10.

For, in this cafe, the quantities Q R and D C will vanifh, and making M R $= m$, the equation, retaining the other fymbols, will be $\overline{m - y}$

$$\times \sqrt{\frac{2rxna + 2rla^2 - x^2 - 2xnla - a^2l^2}{a^2}} = m \times \frac{eb - xb}{e}.$$

And here it is furprifing that the great Doctor, while he was confidering his folid, did not fall upon the one I have explained; but indeed, in fearching after new difcoveries, we are often like thofe, who, groping in the dark, mifs the things that are nearest them.

11. To conclude, if the directrix D N of the bafe be a line of any order n, the fection B P will be of the order $2 n$. *Fig.* 11.

In the equation of the curve directrix D N of the bafe $u^n = A z^n + B z^{n-1} u + C z^{n-2} u^2 + D z^{n-3} u^3$, &c. make the abfciffe D $n = z$, and the ordinate N $n = u$; and draw A Q parallel to Dn, and then, other things being as before, the analogy N O : P O :: N R : M R will be thus expreffed,

$$u + \frac{bx - eb}{e} : y :: u + d + \frac{qnx + qal + qac}{a^2}$$

$$: \frac{nmx + mal + mca}{a^2}; \text{ from which we have } u =$$

$$\frac{eb - bx \times \overline{nmx + mca + mal}}{emal + emnx + m^2ace - a^2ey} + \frac{da^2y + nqxy + qaly + qyac}{mnx + mac - a^2y + mal}.$$

And becaufe D $n = z = l + \frac{nx}{a}$, if we fubftitute thefe values of u and z, in the above general equation, the

the line of the section BP will appear to be of the order 2 n.

And now, my Lord, I have given you some general propofitions about the various sections of this folid, and I have shewn how lines of any order may arife from them; which is a new theory, and perhaps may introduce to fomething farther. I have other things of this fort, that relate to what I have publifhed in the Philofophical Tranfactions, Vol. 39. in the year 1735, which I have had many years by me, that I intend to fend you, if my health, and the circumftances of my life, will allow me to revife them. And in the mean time, with great refpect, I am,

My Lord,

Your Lordfhip's moft obedient

and affectionate fervant,

Sion College, Dec. 18, 1759.   W^m. Brakenridge.

E N D *of* P A R T I.

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